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ART. XIV.—*Discovery of Oxygen in the Sun by Photography, and a new Theory of the Solar Spectrum;** by Professor HENRY DRAPER, M.D.

I PROPOSE in this preliminary paper to indicate the means by which I have discovered oxygen and probably nitrogen in the sun, and also to present a new view of the constitution of the solar spectrum.

Oxygen discloses itself by bright lines or bands in the solar spectrum, and does not give dark absorption lines like the metals. We must therefore change our theory of the solar spectrum and no longer regard it merely as a continuous spectrum with certain rays absorbed by a layer of ignited metallic vapors, but as having also bright lines and bands superposed on the background of continuous spectrum. Such a conception not only opens the way to the discovery of others of the non-metals, sulphur, phosphorus, selenium, chlorine, bromine, iodine, fluorine, carbon, etc., but also may account for some of the so-called dark lines by regarding them as intervals between bright lines.

It must be distinctly understood that in speaking of the solar spectrum here, I do not mean the spectrum of any limited area upon the disc or margin of the sun, but the spectrum of light from the whole disc. I have not used an image of the sun upon the slit of the spectrocope, but have employed the beam reflected from the flat mirror of the heliostat without any condenser.

* Read before the American Philosophical Society, July 20th, 1877.

In support of the above assertions the accompanying photograph of the solar spectrum with a comparison spectrum of air, and also with some of the lines of iron and aluminium, is introduced. The photograph itself is absolutely free from handwork or retouching. It is difficult to bring out in a single photograph the best points of these various substances, and I have therefore selected from the collection of original negatives that one which shows the oxygen coincidences most plainly. There are so many variables among the conditions which conspire for the production of a spectrum, that many photographs must be taken to exhaust the best combinations. The pressure of the gas, the strength of the original current, the number of Leyden jars, the separation and nature of the terminals, the number of sparks per minute and the duration of the interruption in each spark, are examples of these variables.

In the photograph the upper spectrum is that of the sun and above it are the wave-lengths of some of the lines to serve as reference numbers. The wave-lengths used in this paper have been taken partly from Angström and partly from my photograph of the diffraction spectrum published in 1872. The lower spectrum is that of the open air Leyden spark, the terminals being one of iron and the other of aluminium. I have photographed oxygen, nitrogen, hydrogen and carbonic acid as well as other gases in Plücker's tubes, and also in an apparatus in which the pressure could be varied, but for the present illustration the open air spark was, all things considered, best. By other arrangements the nitrogen lines can readily be made as sharp as the oxygen are here, and the iron lines may be increased in number and distinctness. For the metals the electric arc gives the best photographic results as Lockyer has so well shown, but as my object was only to prove by the iron lines that the spectra had not shifted laterally past one another, those that are here shown at 4325, 4307, 4271, 4063, 4045, suffice. In the original collodion negative many more can be seen. Below the lower spectrum are the symbols for oxygen, nitrogen, iron and aluminium.

No close observation is needed to demonstrate to even the most casual observer, that the oxygen lines are found in the sun as bright lines, while the iron lines have dark representatives. The bright iron line at G (4307), on account of the intentional overlapping of the two spectra can be seen passing up into the dark absorption line in the sun. At the same time the quadruple oxygen line between 4345 and 4350 coincides exactly with the bright group in the solar spectrum above. This oxygen group alone is almost sufficient to prove the presence of oxygen in the sun, for not only does each of the four components have a representative in the solar spectrum, but the rela-

tive strength and the general aspect of the lines in each case is similar. I do not think that in comparisons of the spectra of the elements and sun, enough stress has been laid on the general appearance of lines apart from their mere position; in photographic representations this point is very prominent. The fine double line at 4319, 4317, is plainly represented in the sun. Again there is a remarkable coincidence in the double line at 4190, 4184. The line at 4133 is very distinctly marked. The strongest oxygen line is the triple one at 4076, 4072, 4069, and here again a fine coincidence is seen though the air spectrum seems proportionately stronger than the solar. But it must be remembered that the solar spectrum has suffered from the transmission through our atmosphere, and this effect is plainest in the absorption at the ultra-violet and violet regions of the spectrum. From some experiments I made in the summer of 1873, it appeared that this local absorption is so great when a maximum thickness of air intervenes, that the exposure necessary to obtain the ultra-violet spectrum at sunset was two hundred times as long as at mid-day. I was at that time seeking for atmospheric lines above H, like those at the red end of the spectrum, but it turned out that the absorptive action at the more refrangible end is a progressive enfeebling as if a wedge of neutral tinted glass were being drawn lengthwise along the spectrum toward the less refrangible end.

I shall not attempt at this time to give a complete list of the oxygen lines with their wave-lengths accurately determined, and it will be noticed that some lines in the air spectrum which have bright analogues in the sun are not marked with the symbol of oxygen. This is because there has not yet been an opportunity to make the necessary detailed comparisons. In order to be certain that a line belongs to oxygen, I have compared, under various pressures, the spectra of air, oxygen, nitrogen, carbonic acid, carburetted hydrogen, hydrogen, and cyanogen. When these gases were in Plücker's tubes a double series of photographs has been needed, one set taken with and the other without Leyden jars.

As to the spectrum of nitrogen and the existence of this element in the sun there is not yet certainty. Nevertheless even by comparing the diffused nitrogen lines of this particular photograph, in which nitrogen has been sacrificed to get the best effect for oxygen, the character of the evidence appears. The triple band between 4240, 4227, if traced upward into the sun has approximate representatives. Again at 4041, the same thing is seen, the solar bright line being especially marked. In another photograph the heavy line at 3995, which in this picture is opposite an insufficiently exposed part of the solar spectrum shows a comparison band in the sun.

The reason I did not use air in an exhausted Plücker's tube for the production of a photograph to illustrate this paper, and thus get both oxygen and nitrogen lines well defined at the same time, was partly because a brighter light can be obtained with the open air spark on account of the stronger current that can be used. This permits the slit to be more closed and of course gives a sharper picture. Besides the open air spark enabled me to employ an iron terminal and thus avoid any error arising from accidental displacement of the reference spectrum. In Plücker's tubes with a Leyden spark the nitrogen lines are as plain as those of oxygen here. As far as I have seen oxygen does not exhibit the change in the character of its lines that is so remarkable in hydrogen under the influence of pressure as shown by Frankland and Lockyer.

The bright lines of oxygen in the spectrum of the solar disc have not been hitherto perceived probably from the fact that in eye-observations bright lines on a less bright background do not make the impression on the mind that dark lines do. When attention is called to their presence they are readily enough seen even without the aid of a reference spectrum. The photograph, however, brings them into greater prominence. From purely theoretical considerations derived from terrestrial chemistry and the nebular hypothesis, the presence of oxygen in the sun might have been strongly suspected, for this element is currently stated to form eight-ninths of the water of the globe, one-third of the crust of the earth and one-fifth of the air and should therefore probably be a large constituent of every member of solar system. On the other hand the discovery of oxygen and probably other non-metals in the sun gives increased strength to the nebular hypothesis, because to many persons the absence of this important group has presented a considerable difficulty.

At first sight it seems rather difficult to believe that an ignited gas in the solar envelope should not be indicated by dark lines in the solar spectrum, and should appear not to act under the law, "a gas when ignited absorbs rays of the same refrangibility as those it emits." But in fact the substances hitherto investigated in the sun are really metallic vapors, hydrogen probably coming under that rule. The non-metals obviously may behave differently. It is easy to speculate on the causes of such behavior, and it may be suggested that the reason of the non-appearance of a dark line may be, that the intensity of the light from a great thickness of ignited oxygen overpowers the effect of the photosphere, just as if a person were to look at a candle flame through a yard thickness of ignited sodium vapor, he would only see bright sodium lines and no dark absorption lines. Of course such an explanation

would necessitate the hypothesis that ignited gases, such as oxygen, give forth a relatively large proportion of the solar light. In the outburst of T *Coronæ*, Huggins showed that hydrogen could give bright lines on a background of spectrum analogous to that of the sun.

However all that may be, I have no doubt of the existence of substances, other than oxygen, in the sun, which are only indicated by bright lines. Attention may be called to the bright bands near G, from wave-length 4307 to 4337, which are only partly accounted for by oxygen. Further investigation in the direction I have thus far pursued will lead to the discovery of other elements in the sun, but it is not proper to conceal the principle on which such researches are to be conducted for the sake of personal advantage. It is also probable that this research may furnish the key to the enigma of the D_3 or helium line and the 1474 K or corona line. The case of the D_3 line strengthens the argument in favor of the apparent exemption of certain substances from the common law of the relation of emission and absorption, for while there can be no doubt of the existence of an ignited gas in the chromosphere giving this line, there is no corresponding dark line in the spectrum of the solar disc.

In thus extending the number of elements found in the sun we also increase the field of inquiry as to the phenomena of dissociation and recomposition. Oxygen especially, from its relation to the metals, may readily form compounds in the upper regions of the solar atmosphere which can give banded or channelled spectra. This subject requires careful investigation. The diffused and reflected light of the outer corona could be caused by such bodies cooled below the self-luminous point.

This research has proved to be more tedious and difficult than would be supposed, because so many conditions must conspire to produce a good photograph. There must be a uniform prime moving engine of two horse power, a dynamo-electric machine thoroughly adjusted, a large Ruhmkorff coil with its Foucault break in the best order, a battery of Leyden jars carefully proportioned to the Plücker's tube in use, a heliostat which of course involves clear sunshine, an optical train of slit, prisms, lenses and camera well focussed, and in addition to all this a photographic laboratory in such complete condition that wet sensitive plates can be prepared, which will bear an exposure of fifteen minutes and a prolonged development. It has been difficult to keep the Plücker's tubes in order; often before the first exposure of a tube was over the tube was ruined by the strong Leyden sparks. Moreover to procure tubes of known contents is troublesome. For example, my

hydrogen tubes gave a spectrum photograph of fifteen lines, of which only three belonged to hydrogen. In order to be sure that none of these were new hydrogen lines it was necessary to try tubes of various makers, to prepare pure hydrogen and employ that, to examine the spectrum of water, and finally to resort to comparison with the sun.

The object in view, in 1873, at the commencement of this research, was to secure the means of interpreting the photographs of the spectra of stars and other heavenly bodies, obtained with my 28-inch reflector. It soon appeared that the spectra of nitrogen and other gases in Plücker's tubes could be photographed, and at first some pictures of hydrogen, carbonic acid and nitrogen were made, because these gases seemed to be of greatest astronomical importance on account of their relation to stars, nebulæ and comets. Before the subject of comparison spectra of the sun was carefully examined, there was some confusion in the results, but by using hydrogen the source of these errors was found out.

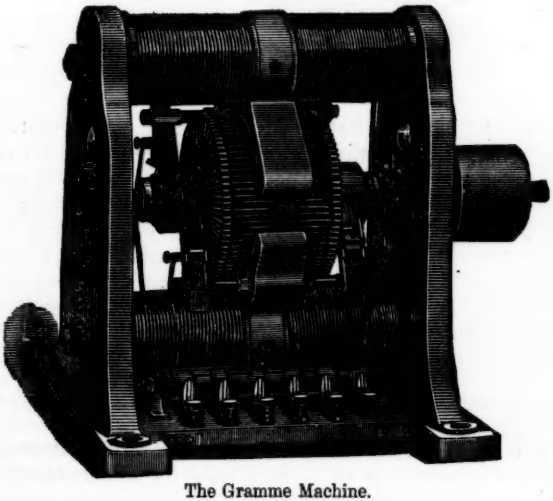
But in attempting to make a prolonged research in this direction it soon appeared that it was essential to be able to control the electrical current with precision, both as to quantity and intensity, and, moreover, to have currents which when once adjusted would remain constant for hours together. These conditions are almost impossible to attain with any form of battery, but on the contrary are readily satisfied by dynamo-electric machines. Accordingly I sought for a suitable dynamo-electric machine and motor to drive it, and after many delays procured a combination which is entirely satisfactory. I must here acknowledge my obligations for the successful issue of this search to Professor Geo. F. Barker, who was the first person in America to procure a Gramme machine. He was also the first to use a Brayton engine to drive a Gramme.

The dynamo-electric machine selected is one of Gramme's patent made in Paris and is a double light machine, that is, it has two sets of brushes, and is wound with wire of such a size as to give a current of sufficient intensity for my purposes. It is nominally a 350 candle-light machine, but the current varies in proportion to the rate of rotation, and I have also modified it by changing the interior connections. The machine can produce as a maximum a light equal to 500 standard candles, or by slowing the rotation of the bobbin the current may be made as feeble as that of the weakest battery. In practical use it is sometimes doing the work of more than fifty large Grove nitric acid cells and sometimes the work of a single Smee.

The Gramme machine could not be used to work an induc-

tion coil when it first reached me, because when the whole current was sent through the Foucault interruptor of the Ruhmkorff coil, making 1,000 breaks per minute, the electro-magnets of the Gramme did not become sufficiently magnetized to give an appreciable current. But by dividing the current so that one pair of the metallic brushes, which collect from the revolving bobbin, supplied the electro-magnets, the other pair could be used for exterior work, no matter whether interrupted or constant. The current obtained in this way from one pair of brushes when the Gramme bobbin is making 1,200 revolutions per minute, is equal to one hundred candles, and is greater in quantity and intensity than one would like to send through a valuable induction coil. I usually run the bobbin at 622 revolutions per minute, and this rate will readily give 1,000 ten-inch sparks per minute with the eighteen-inch coil. Of course a Plücker's tube lights up very vividly, and generally, in order to get the maximum effect, I arrange the current so that the aluminium terminals are on the point of melting. The glass, particularly in the capillary part, often gets so hot as to char paper. The general appearance of the machine is shown in fig. 1.

1.

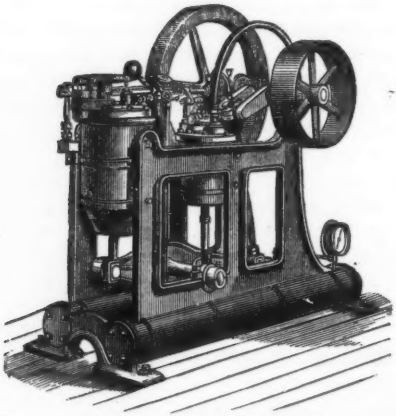


The Gramme Machine.

As long as the Gramme bobbin is driven at a steady rate the current seems to be perfectly constant, but variations of speed make marked differences in the current, and this is especially to be avoided when one is so near the limit of endurance of Plücker's tubes. A reliable and constant motor is therefore

of prime importance for these purposes. A difference of one per cent in the speed of the engine sometimes cannot be tolerated, and yet at another time one must have the power of increasing and diminishing through wide limits. The only motor, among many I have examined and tried, that is perfectly satisfactory, is Brayton's Petroleum Ready Motor. This remarkable and admirable engine acts like an instrument of

2.



Brayton's Petroleum Motor.

precision. It can be started with a match, and comes to its regular speed in less than a minute; it preserves its rate entirely unchanged for hours together. Moreover, it is economical, cleanly and not more noisy than a steam engine. The one of two horse power I have ran for six months, day and night, supplying water and air to the aquaria in the Centennial Exhibition at Philadelphia. At any time on going into the laboratory it can be started in a few seconds, even though it has not been running for days.

Henry Draper's Observatory, Hastings-on-Hudson, New York.

ART. XV.—*On the Theory of the Action of certain Organic Substances, in increasing the Sensitiveness of Silver Haloids;* by M. C. LEA.

IN the early days of photo-chemistry, it was observed that all the silver haloids gained in sensitiveness by being in contact with a soluble silver salt; that when the soluble silver salt was removed by careful washing, a considerable diminution in sen-

sitiveness followed. After a time it was noticed that certain organic substances placed in contact with the washed haloid, appeared to take, to some extent, the place of the silver salt removed, and that the silver haloid was distinctly more sensitive when in contact, for example, with tannin, than when isolated.

In explanation of the fact, a theory was published by M. Poitevin that these organic substances acted in virtue of their affinity for the halogen. That in the case, for example, of tannin and silver iodide, the iodide was more readily attacked by light in the presence of tannin, because the affinity of tannin for iodine came into play in aid of the action of light. Shortly after, Dr. H. Vogel published the same theory.

It is a little remarkable that neither of the authors of this theory, or rather hypothesis, has at any time thought it necessary to support it by experiment or proof of any kind, but both have continued up to the present time to treat the theory as self-evident. In endeavoring to show that this explanation of the reaction cannot be received, I shall have no argument on the other side to combat, or experiments to discuss, as I can find none. I shall, therefore, state what I conceive to be the true explanation, and endeavor to support it adequately by proof.

When we examine all the substances known to be capable of increasing the sensitiveness of the silver haloids, we notice that the one property which they possess in common is, *not* an affinity for the halogen, as the Poitevin–Vogel theory would lead us to expect, but that they *are all reducing agents.* The natural view, therefore, of their action on a silver haloid is, not that they abstract the halogen from it, but that their affinity for oxygen comes into aid of the affinity of the halogen for hydrogen, and that under the influence of light, an atom of water is decomposed. That in the case of silver iodide and tannin, for example, the tannin is oxidized, the iodine is converted into hydriodic acid, and the silver salt is more or less completely reduced, probably to subiodide. According to this view, when silver iodide is exposed to light in presence of an organic body capable of accelerating the action of light, there should be traces of free acid formed, whereas, according to the Poitevin–Vogel theory the result of the action should be the formation of an iodo-substitution compound.

1. Silver iodide was precipitated in the presence of a slight excess of potassium iodide, and thoroughly washed. A small quantity of pyrogallol was added and the mixture was exposed to sunlight for fifteen minutes in contact with water. The liquid at first neutral, at the end of fifteen minutes, showed a distinctly acid reaction, increasing with continued exposure.

[Pyrogallol was used in preference to tannin (gallotannic

acid) because the former is perfectly neutral to test paper, and cannot confuse the reaction as would be the case with tannin, which of itself reddens blue litmus paper].

Again: if the Poitevin–Vogel theory were correct, it would afford a criterion as to what substances should, and what should not, exalt the sensitiveness of the silver haloids. Substances having an affinity for iodine should increase the sensitiveness, and substance not possessing such affinity, should have no such action. This is not the fact. There are substances which exalt an affinity for iodine, that do not exalt the sensitiveness of silver iodide and conversely, for example:

2. Silver iodide was exposed to light for a month under a solution of potassium carbonate, in a test tube, with another test tube, for comparison, containing silver iodide under water only. The alkali did not in the least increase the tendency of the iodide to darken.

3. Starch, which has a well-known affinity for iodine, does not appear to increase the sensitiveness of silver iodide by contact with it.

These experimental results appear to be incompatible with the Poitevin–Vogel theory.

The view which I have here offered is, moreover, very strongly supported by the phenomena of alkaline development. When a sensitive surface has been exposed to light, the image may be developed by pyrogallol, a neutral organic substance. This development is greatly stimulated by the presence of an alkali, a fact which the Poitevin–Vogel theory leaves unexplained. But according to the view here proposed, viz: that the result of the action of the organic body is not a substitution product, but a halogen acid, (e. g. HI, etc.), it is natural that the presence of an alkali should increase the tendency to form an acid for which it has a strong affinity.

Also, this new explanation is in harmony with received views of the action of the halogens in analogous cases, and particularly with that of the action of chlorine in bleaching organic substances. The modern view of the action is, not that the chlorine attracts the organic body with formation of a substitution compound, but that, by the coöperating affinity of the organic substance for oxygen, and that of the chlorine for hydrogen, an atom of water is decomposed, with formation of hydrochloric acid and peroxide of hydrogen, which latter substance attacks the oxidizable organic body. It will be observed that the reaction is exactly the same in both cases, the bleaching and the photo chemical. In fact, the parallelism is in all respects as complete as it can possibly be, and the analogy lends a strong support to the view that I have here expressed.

I conclude, therefore, that such organic bodies as increase

the sensitiveness of the silver haloids to light, do so, not by forming substitution compounds with the halogen, but by promoting, in virtue of their affinity for oxygen, the decomposition of water by the halogen.

There is, probably, another affinity which plays some part in the tendency to form the halogen acid, and that is the tendency shown by the silver haloid to form combinations with the corresponding halogen acids, e. g. hydroargentic iodide AgIHI, etc., in the absence of an alkali.

Philadelphia, June 26, 1877.

ART. XVI.—*On Critical Periods in the History of the Earth and their Relation to Evolution: and on the Quaternary as such a Period;** by JOSEPH LECONTE.

IN the series of rocks representing the history of the earth there occur at different horizons, *unconformities*. In most cases these do not occur at the same horizon in different places; but there are a few which seem to be very general. Associated with these unconformities, as is well known, there is nearly always a marked change in the fossil species. The greatness of this change is always in direct proportion to the generality of the unconformity. These general unconformities attended with very great changes in organic forms are the natural boundaries of the great divisions of time, and the less general unconformities attended with less sweeping change of organic forms, of the subdivisions.

The earlier geologists, under the influence of the *then* dominant idea of frequent supernatural interference with the course of nature, imagined that these unconformities marked the times of instantaneous cataclysm which disturbed the rocks and destroyed all living things, sometimes locally, sometimes generally; and that these exterminations were followed by re-creations of other and wholly different species at the beginning of the subsequent period of tranquility. *Now*, however, we believe that no such instantaneous general exterminations and re-creations ever occurred. Now we know that unconformity simply indicates eroded land-surface, and therefore marks a period of time during which the observed place was land and received no sediment—that two series of rocks unconformable to each other indicate two periods of comparative quiet, during which the observed place was sea-bottom receiving sediment steadily, separated by a period of oscillation producing increase and decrease of land, during which the observed place

* Read before the National Academy of Sciences, April 18, 1877.

was raised into land-surface with or without crumpling of the strata, deeply eroded and then sunk again below sea level to receive the second series of strata. The length of the two periods of repose is roughly measured by the thickness of the two conformable series. The length of the period of commotion is roughly measured by the amount of erosion at the line of unconformity.

Evidently, therefore, every case of unconformity marks a period of time, often a long period, during which there was no record made in strata and fossils at the observed place; certain leaves, often very many, are there missing from the Book of Time. Is it any wonder then, that skipping over these pages when we commence reading again we find the matter entirely new? Evidently, then, the suddenness of the change in organic forms is only apparent. If we could recover the record, which was doubtless carried on elsewhere, the break would disappear; if we could recover the missing leaves the reading would be continuous. In every such case therefore there is a *lost interval* of history. In cases of local unconformity we recover the lost record in other places, and thus fill up the blank in the history. But in some cases of very general unconformity, such as those which mark the great divisions of time, the loss is not yet recovered, perhaps is irrecoverable; though doubtless the more complete knowledge of the geology of the whole earth-surface will go far toward filling blanks and making the record continuous.

The view above presented is now held by all geologists, but there seems to be danger, under the influence of the *now* dominant views of evolution, of erring on the other extreme. Assuming a *uniform rate* of evolution, many, it seems to me, commit the mistake of measuring the amount of lost interval by the amount of change of organic forms, and thus discredit the real value of the geological record by exaggerating greatly its fragmentary character. On the contrary there seems good reason to believe that the evolution of the organic kingdom, like the evolution of society and even of the individual, has its periods of *rapid movement*, and its periods of *comparative repose* and re-adjustment of equilibrium. Geological history, like all other history, has its periods of comparative quiet, during which the forces of change are gathering strength; and periods of revolution during which the accumulated forces manifest themselves in conspicuous changes in physical geography and climate, and therefore in rapid movement in the march of evolution of organic forms—periods when the forces of change are *potential*, and periods when they become *active*. Conformable rocks represent the periods of comparative quiet, during which organic forms are either permanent or change slowly; uncon-

formity represents a time of oscillation with increase and decrease of land, and therefore of rapid changes of physical conditions and correspondingly rapid movement in evolution. The general unconformities, of course, mark times of very general commotion—of wide-spread changes of physical geography and climate, and therefore of exceptionally rapid and profound changes in organic forms.

These periods of revolution in all history are *critical;* and therefore are of especial interest to the philosophic historian and to the evolutionist; but they are also in all history periods of *lost record.* And as in human, so also in geological history, the farther back we go, the longer are the lost intervals and the more irrecoverable the lost records. We will now give examples of such lost intervals and show their significance in evolution.

The first and by far the greatest of these lost intervals is that which occurs between the Archæan and the Paleozoic. In every part of the earth where the contact has been yet observed the Primordial lies unconformably on the upturned and eroded edges of the Archæan strata. This relation was observed first in Canada, then in various parts of the eastern United States, then in Scotland, Hebrides, Bavaria, Bohemia, Scandinavia. Unconformity in such widely separated localities, indicates wide-spread changes in physical geography, and therefore presumably of all those physical conditions included in the word *climate.* These changes of physical geography are best illustrated in the United States. The break between the Archæan and the Primordial has been observed in very many places all over the wide area of the United States east and west; not only in Canada, in New York, in the Appalachian region, in Wisconsin, Missouri, Arkansas and Texas, but also all over the Rocky Mountain region, in Nebraska, Montana, Idaho, Wyoming, Colorado, Utah, Nevada, New Mexico and Arizona. As upturned eroded outcropping strata mean land-surface, it is evident that there was at that time *a very large area, or else several large areas of land,* in the place now occupied by the American Continent. In comparison with the subsequent Silurian it was *a continental Period.* This land is often spoken of as *Archæan land.* It was indeed land of *Archæan rocks,* but for that very reason not of Archæan times: for these rocks were of course formed at the bottom of the sea in Archæan times, and therefore these localities were all sea-bed receiving sediment at that time. We know absolutely nothing of the land of Archæan times, and never can know anything until we find still older rocks from the debris of which Archæan sediment, were formed. The land spoken of above, was *land of the Lost Interval.* That the interval was immensely long is evident from the prodigious

erosion. That it was a period of wide-spread oscillation is also evident; for all the places mentioned were sea-bed in Archæan, land during the interval, and again sea-bed during the Silurian. But of this long interval not a leaf of record remains.

Evidently then at the end of the Archæan an enormous area of Archæan sea-bottom was raised up and crumpled and became land. After remaining land for a time sufficiently long to allow enormous erosion of the crumpled strata it again went down to the old Primordial shore-line and the Silurian age commenced. This time of elevation is the lost interval.

Now, when the record closed in the Archæan, as far as we know, only the lowest forms of Protozoan life yet existed. The beginnings of life had not yet differentiated into what might be called a fauna and flora. When the record again opened with the Primordial we have already a varied and highly organized fauna, consisting of representatives of many classes and of all the great types of animal structure, except vertebrates. Nor are these representative the lowest in three several departments; for Trilobites and Orthoceratites can hardly be regarded as lower than the *middle of the animal scale* as it now exists. It is certain therefore that all the great departments except vertebrates, and most of the classes of these departments including animals as least half way up the animal scale, were differentiated during the lost interval. The amount of evolution during this interval cannot be estimated as less than all that has taken place since. Measured by the amount of evolution therefore, this lost interval is equal to all the history of the earth which has elapsed since. We escape this very improbable conclusion, only by admitting *a more rapid rate of evolution during critical periods.*

It is one of the chief glories of American Geology, to have first established the Archæan as one of the primary divisions of time. It is even yet reluctantly admitted as such by many European geologists. And yet, it is seen, that from every point of view, whether of the rock system or of the life system, it is by far the most widely and trenchantly separated of all the eras.

The next greatest lost interval (though far less than the preceding) is that between the Paleozoic and the Mesozoic. Here we have the next most general unconformity, indicating the next most wide-spread changes of physical geography and climate, accompanied by the most sweeping changes in organic forms, not only in species and genera, but also in families and orders. This change is the more striking as it occurs in the midst of an abundant life. It is the greatest and most sweeping change in the forms of organisms which has ever occurred in the history of the earth. *It took place, again, during a lost in-*

terval. A portion of the loss is recovered in the Permian; but the most critical time, the time of most rapid change, viz: that between the Permian and the Trias, is still missing. How we long to find the steps of this great change! What a flood of light would it shed on the process of evolution! But although the change in the organic kingdom was, just here, so enormously great, yet the lost interval does not seem very long; for in England the Trias and Permian seem to be conformable, though probably with change from marine to fresh water conditions. It is impossible therefore to resist the conclusion that the steps were just here fewer and longer, and the progress more rapid than usual. As in human history revolutions are the times of the birth of new social ideas, upon which during the subsequent period of tranquillity, society is re-adjusted in prosperity and happiness on a higher plane; so also in geological history, critical periods are times of origin of new and higher organic forms and the subsequent periods of tranquillity are times of re-adjustment of equilibrium and prosperous development of these forms.

Like the previous lost interval, this was also a period of oscillation—a period of great increase of land, which was again partly submerged to inaugurate the Trias. It was therefore, again, *a continental period.* The land-making commenced at the end of the Coal period, in this country with the formation of the Appalachian Mountains, continued through the Permian and culminated in the lost interval, which is in fact for that very reason lost.

Far less in length of time and perhaps in the sweeping character of the change of organisms, but far more important and interesting on account of the high position of the animals involved is the lost interval between the Mesozoic and Cenozoic. The length of time lost here is comparatively small. In America in many parts of the west the uppermost Cretaceous seems to pass into the lowermost Tertiary without the slightest break of continuity. There may be some break, some unconformity, some lost record, but certainly it cannot be large. Yet the change especially in the higher animals is immense. In America the break and the lost interval is much greater between the Jurassic and Cretaceous than between the Cretaceous and Tertiary, yet the organic change is far greater in the latter case. The reason is that the changes of physical geography and climate in the latter were *more general.* Although in America the break and the lost interval is greater at the end of the Jurassic, yet taking the strata all over the earth, the break is far more general at the end of the Cretaceous; and it is these *general* changes in physical geography which affect climate the most and which therefore produce the profoundest changes in organic forms.

Now it is almost impossible to imagine a clearer proof of the fact of rapid evolution-movement during critical periods, than we find in the shortness of the lost interval and yet the greatness of the change in higher organisms just at this horizon in the rocky series. Nothing can be more astonishing than the abundance, variety and prodigious size of Reptiles in America up to the very close of the Cretaceous, and the complete absence of all the grander and more characteristic forms in the lowest Tertiary; unless, indeed, it be the correlative fact of the complete absence of mammals in the Cretaceous and their appearance in great numbers and variety in the lowest Tertiary. If Cretaceous mammals existed in *America*, surely their remains would have been found in the wonderfully rich Cretaceous strata. It seems certain that in America, or at least in that portion which has been examined, mammals appeared somewhat suddenly and in great numbers on the scene and were a principal agent in the extermination of the great reptiles. The wave of reptilian evolution had just risen to its crest and perhaps was ready to break, when it was met and overwhelmed by the rising wave of mammalian evolution.

We have dwelt only on the great change in the higher classes, but the change really extended to all classes. This was therefore a time of exceptionally general and rapid changes in all departments alike. In other words it was a critical period in organic evolution.

That it was also a time of very great changes in Physical geography, here in America as well as elsewhere, is well known. The Cretaceous sea which extended from the Gulf of Mexico to the Arctic Ocean, covering the whole western plains and plateau region, and thus divided the American continent into two, an eastern Appalachian continent and a western or Basin region continent, was abolished at the end of the Cretaceous, and replaced by great fresh water lakes in the same region, and the continent became one. Moreover it is probable that it was a period of wide-spread oscillation, i. e., of upheaval and again subsidence to the condition of things found at the beginning of the Tertiary. It is probable that the upheaval which abolished the Cretaceous sea went much beyond the condition of things afterwards—that just at the interval the land was higher and larger than in the Tertiary—that, in short, this was again a *continental period* and probably a period of greater cold than the subsequent Tertiary.

The change in physical geography, then, was immense, but in most places by bodily upheaval, not by crumpling of the strata, and therefore the usual signs of such change, viz: unconformity is often wanting. The change of climate all over the American continent was no doubt very great and the

change in organic forms correspondingly great everywhere and in all departments; but this was especially true of all water-inhabiting species, in the region of the old Cretaceous interior sea; for here there was a change not only in climate but from salt to fresh water through the intermediate condition of brackish water. The Cretaceous marine species rapidly disappeared, partly by extermination and partly by transmutation into fresh water species, as has been observed, recently, to take place in some crustaceans under this change of conditions.* The Tertiary fresh-water species rapidly appeared partly by transmutation from the previous marine and partly by transportation in various ways from other fresh lakes. But all this occurred in some places without the slightest break in the continuity of the strata.

The great change of climate and other physical conditions perhaps sufficiently explain the change in *invertebrate* species, but it is impossible to account for the somewhat sudden appearance of mammals in the lowest Tertiary except by *migration* from other regions where they had existed in late Cretaceous times, having originated there by derivation in the usual way. That marsupials existed somewhere in Cretaceous times (though possibly not in America or Europe) there can be no doubt; for they existed we know in the preceding Jurassic and the following Tertiary, and they exist *now*. It is from these rather than from Cretaceous reptiles, that Tertiary mammals were doubtless derived; and this derivation took place probably at rapid rate in the latest Cretaceous or during the lost interval in some unknown locality whence they migrated into the Tertiary lake region of the United States during the interval. This migration came most probably from northern Asia; for it must be remembered that the interval was a continental period and therefore probably a period of broad land connections between Nearctic and Palearctic regions. The complete examination of the uppermost Cretaceous of different portions of Asia will probably reveal the immediate progenitors of the early Tertiary mammals of Europe and America. This introduces us to a most important element of rapid local faunal change especially in higher animals, viz: *migrations*. If we do not dwell longer now on this, it is only because we shall have to recur to it again.

I have preferred, thus far, to speak of *general* evolution-changes of organisms, whether slow or rapid, as produced by varying pressure of external conditions; and the most striking *local* changes by migrations from other regions where the apparently suddenly-appearing species had previously existed, having originated there by evolution in the usual way. I have

* Arch. des Sciences, Nov., 1875, p. 284.

preferred, thus far, to represent the organic kingdom as lying, as it were, *passive* and plastic under the moulding hands of the environment. I have done so because it is in accordance with true method to exhaust the more obvious causes of evolution before appealing to the more obscure and doubtful.

It is possible that general movements affecting alike all classes may be accounted for in this way alone. But there are many facts in the evolution of the organic kingdom, especially the sudden appearance of new forms in the quietest times, which can hardly be thus explained. There seem to be *internal* as well as *external* factors of evolution. Again the internal factors may be either in the form of *tendencies* to change or of *resistance* to change. Of these, however, the latter seems to be most certain. There may be in the organic kingdom an "*inherent tendency*" to change in special directions, similar to that which directs the course of embryonic evolution—a tendency, in the case of the organic kingdom, inherited from physical nature from which it sprung, as in the case of the embryo it is inherited from the organic kingdom through the line of ancestry. This cause, however, is too obscure, and I therefore pass it by.

But whether or not there be any such inherent tendency to change, there certainly is an inherent tendency to *stability*—to persistence of organic form. If there be no inherent force of progress, there certainly is an inherent force of *conservation* greater in some species than others. It seems probable that in many of the more rigid types this stability is so great, and therefore variation of offspring is so slight, that progressive change of form is too slow to keep pace with change of external conditions especially in critical periods. If this be so, then an organism may be regarded as under the influence of two opposing forces, the one conservative, the other progressive; the one tending to equilibrium, the other to motion; the one to permanence, the other to change of form; the one static, the other dynamic; the one internal, *the law of heredity*, the other external, the *pressure of a changing environment*. Under the influence of two such forces, the one urging, the other resisting, it is evident that even with steady changes of external conditions, the change of organic forms would be more or less paroxysmal. Other kinds of evolution, physical and social, evidently advance paroxysmally from this cause. As therefore in the gradual evolution of earth-features there are periods of comparative quiet, during which the forces of change are gathering strength, but produce little visible effect, being resisted by crust-rigidity, and periods when the accumulating forces finally overcome resistance and determine comparatively rapid changes; as also in social evolution there are periods in which forces of

social change are gathering strength, but make no visible sign, being resisted by social conservatism—rigidity of the social crust—and periods in which resistance gives way and rapid changes occur; so also in the evolution of the organic kingdom the forces of change, i. e. pressure of changing environment, may accumulate but make little impression, being resisted by the *law of heredity*—of *like producing like*—or type-rigidity, untill finally the resistance giving way, the organic form breaks into fantastic *sports* which are at once seized by natural selection and rapid change is the result.

Some persons seem to think that paroxysmal evolution is inconsistent with the uniformity of Nature's laws. On the contrary, it is in perfect accord. Laws and forces are indeed uniform, but phenomena are nearly always paroxysmal. The forces of volcanos and earthquakes, of lightning and tempests are uniform, but the phenomena are paroxysmal. Winds at the earth's surface, where the resistance is great, *blow in puffs*. A thin sheet of water over a smooth sloping surface *runs in waves*. The law may be illustrated in a thousand ways. In all cases where an accumulating force is opposed by a constant resistance, we have phenomena in paroxysms.

But whatever be the cause, the *fact* of paroxysmal movement of organic evolution is undoubted. All along the course of geological history, from beginning to end, even when the times were quietest, where the record is fullest and apparently without any missing leaf, species come and go and others take their place, and yet only rarely do we find any transition-steps. If this were only once or twice or thrice, or to any extent exceptional, it might be explained by loss of record here and there, but it occurs thousands and tens of thousands of times. Now if evolution moves only at uniform rate—if it takes 100,000 years to transmute one species into another (as it certainly does when evolution is moving at its usual rate)—if there are at least 100,000 steps (represented each of course by a whole generation of many individuals), between every two consecutive species, it is simply incredible that all the individuals representing the intermediate steps, so infinitely more numerous than the species they connect, should be so generally, almost universally lost. But the phenomena as we find them, are easily understood, if a few generations represent the transition step, and many generations the permanent form.

A similar rapid, almost sudden appearance and extinction of *genera*, *families* and higher groups at certain horizons, are also common. In these cases the intermediate steps of transition are often found, and constitute in fact the chief demonstrative evidence of the truth of evolution. But the difficulty on the assumption of a uniform rate of evolution is none the less here,

for the time required to evolve a new genus or a new family is of course immensely greater than in the case of a new species.

We will illustrate the difficulties of the ordinary view by one striking example. In the Upper Silurian in the midst of a conformable series, where if there be any break—any lost record—surely it must be very small, appear suddenly, without premonition, *Fishes:* not a connecting link between fishes and any form of invertebrates, but perfect, unmistakable fishes. Here we have, therefore, the appearance not only of a new class, but of a new sub-kingdom or type of structure, *Vertebrata.* Now to change from any previously existing form of invertebrate, whether worm, crustacean or mollusk, into a vertebrate, by a series of imperceptible steps represented by successive generations—steps so imperceptible that it would take 100,000 of them to advance from one intermediate species to another—would require an amount of time which is inconceivable to the human mind, and a number of steps, each be it remembered, represented by thousands of individuals, which can scarcely be expressed by figures. And yet we must believe that these innumerable transitional forms, each represented by innumerable individuals, are all lost, and that this prodigious time shows no evidence in the rocky record. If this case were exceptional we might possibly admit that fishes appeared in Great Britain by migration (as they probably did), but only after having previously existed untold millions of ages, somewhere else; but similar cases are too common to be explained in this way.

Now the whole difficulty disappears—we avoid the incredible imperfection of the geological record (imperfect at best)—we avoid also the necessity of extending geological time to a degree which cannot be accepted by the physicist—if we admit that the derivation of one species from another is not necessarily by innumerable imperceptible steps, but may sometimes be by a *few decided steps;* and that the same is true for the origin of new genera, families, orders, etc.; in a word, that there are in the history of evolution of species, genera, families, orders, etc., and of the organic kingdom, *periods of rapid movement.* When the whole organic kingdom is involved in the movement, then we call the period *critical,* and the record of it is often lost.

Thus, on the supposition of such rigidity or resistance to change, in organic forms, varying in degree in different species and in different genera, families, orders, etc., a rigidity, also, *increasing by accumulated heredity so long as conditions remain unchanged,* it is evident that, in times of perfect tranquility all species grow more and more rigid. In times of very gradual change, the more plastic species change gradually *pari passu,* while the more rigid species change paroxysmally, now one,

now another, as their resistance is overcome. Finally, in times of revolution nearly all forms yield to the pressure of external conditions and change rapidly; *only the very exceptionally rigid being able to pass over the interval to the next period of re-adjusted equilibrium.*

Thus, for example, the great and wide-spread changes of physical geography which occurred at the end of the Carboniferous, appropriately called in this country, the *Appalachian revolution,* was the death-sentence of the long-continuing and therefore rigid Paleozoic types. But the sentence was not immediately executed. The Permian represents the time between the sentence and the execution—the time during which the more rigid Paleozoic forms continued to linger out a painful existence in spite of changed and still changing conditions. But the most critical time—the time of most rapid change—the time of actual execution—was the *lost interval.* Only a very few most rigid forms pass over this interval into the Trias.

The Quaternary, a Critical Period.

We have given examples of several general unconformities, the signs of wide-spread oscillations of the earth-crust, attended with increase and decrease of land, and therefore with great and wide-spread changes of climate and other physical conditions, and therefore also with great and rapid changes of organic species. These times of general oscillation are therefore the natural boundaries of the Eras or primary divisions of Time. We have called them critical periods, transition-periods, periods of revolution, because they are times of rapid change, both in the physical and the organic world—a change overthrowing an *old* and establishing a *new* order of things. They are also times of *lost record.* We have seen, also, that these critical periods, in comparison with the preceding and succeeding, are *continental periods,* and it is for this reason that their record is usually lost.

Now, the Quaternary is also such a critical or transition period, marking the boundary between two great Eras. The Quaternary is also a period of great and wide-spread oscillations, with increase and decrease of land—a period of upheaval, erosion, down-sinking, to rise again slowly to the present condition. The early Quaternary was, also, therefore, to a marked degree, a continental period. Here also we have newer rocks lying unconformably on the eroded edges of an older series—river sediments in old river valleys, marine sediments in fiords—in other words, we have unconformity on a grand scale. Also in connection with these oscillations we have great changes in physical geography, and corresponding and very wide-spread changes in climate, and therefore corresponding

rapid changes in organic forms. Here then we have all the characteristics of one of the boundaries between the primary divisions of time. Here then we have a transition or *critical* period—a period corresponding to one of the lost intervals; only in this instance being so recent, and being also less violent than the preceding ones, *it is not lost.* The study of the Quaternary, therefore, ought to furnish the key which will unlock many of the mysteries which now trouble us. Some of the problems which have been or will be explained by study of the Quaternary, we will now briefly mention.

I. *Changes of species not sudden.*—If the Quaternary were lost and we compared the Tertiary rocks with the unconformably overlying recent rocks, and the Tertiary mammals with those now living, how great and apparently sudden seems the change! How like to a violent extermination and re-creation! But the Quaternary is fortunately not lost, and we see that there has been no such wholesale extermination and re-creation, but only gradual though comparatively rapid change.

II. *Migration one chief cause of change.*—But what is still more important, we are able to trace with something like certainty, the cause of these rapid changes, and we find that in the higher animals, chief among these have been *migrations*—migrations enforced by changes of climate, and migrations permitted by changes of physical geography opening gateways between regions previously separated by impassable barriers. This point is so important that we must dwell upon it. Only an outline, however, of some of these migrations and their effects on evolution can be given in the present condition of knowledge.

During Miocene times, as is well known, evergreens, allied to those now inhabiting Southern Europe, covered the whole of Europe as far north as Lapland and Spitzbergen. In America, Magnolias, Taxodiums, Libocedrus and Sequoias very similar to, if not identical with those now living on the Southern Atlantic and Gulf coast and in California, were abundant in Greenland. Evidently there could have been no *Polar ice-cap* at that time and therefore no Arctic species unless on mountain tops. During the latter part of the Pliocene the temperature did not differ much from the present; the polar ice-cap had therefore commenced to form, with its accompaniment of Arctic species. With the coming on of the *Glacial epoch*, the polar ice and Arctic conditions crept slowly southward, pushing Arctic species to middle Europe and Middle United States, and sub-Arctic species to the shores of the Mediterranean and the Gulf. With the return of more genial climate, Arctic conditions went slowly northward again, and with them went Arctic species slowly migrating generation after generation to their present Arctic home.

Similarly molluscous shells migrated slowly southward and again northward to their present position. But *plants* and some terrestrial invertebrates, such as *insects*, had an alternative which shells had not, viz: that of seeking Arctic conditions also *upward* on the tops of mountains. Many did so and were left stranded there until now. It is in this way that we now account for the otherwise inexplicable fact that Alpine species in Middle Europe are similar or even largely identical with those in the United States, and both with those now living in Arctic regions. These species were widespread all over Europe and United States in Glacial times; and while some of them afterward went northward to their present home, some in each country sought Arctic conditions in Alpine isolation. This explanation, which has been long recognized for plants, has been recently applied by Mr. Grote, also to Arctic insects found on the top of Mt. Washington and the mountains of Colorado.*

Undoubtedly changes of climate during this time enforced similar migrations among mammals also. But it is evident that while plants and invertebrates might endure such changes of climate and such enforced migrations with little change of form, the more highly organized and sensitive mammalian species must be either destroyed or else must undergo more profound changes. Moreover, the opening of land-connections between regions previously isolated by barriers would be far more quickly taken advantage of by mammals than by invertebrates and plants. The migrations of plants are of necessity very slow, i. e., from generation to generation. The migrations of mammals too, so far as they are *enforced* by changing climate, are of a similar kind; but the voluntary migrations of mammals *permitted* by removal of barriers may take place much more rapidly—even in a few generations. This introduces another element of very rapid local change, viz: the *invasion* of one fauna by another equally well adapted to the environment, and the struggle for life between the invaders and the autochthones.

For example: in America during the Glacial epoch, coincidently with the rigorous climate, there was an elevation of the continent, greatest in high latitude regions, but also probably greatest along the line of the Mississippi River; for in this region it extended southward even to and beyond the shores of the Gulf. Professor Hilgard has shown that the elevation at the mouth of the Mississippi River was at least 450 to 500 feet above the present condition. Until the Glacial times the two Americas were certainly separated by sea in the region of the Isthmus, as shown by the Tertiary deposits there. This barrier was removed by upheaval during the Glacial epoch,

* This application, with reference to Mount Washington and other Arctic insects in America, was previously made by Prof. A. S. Packard, Jr., in the Memoirs of the Boston Soc. Nat. Hist., i, p. 256, 1867.—EDS.

and a far broader connection existed *then* than now. Through this open gateway came the fauna of South America, especially the great Edentates, into North America. Similarly a broad connection then existed between America and Asia in the regions of the shallow sea between the Aleutian Isles and Behring Straits. Through this gateway came an invasion from Asia, including probably the Mammoth. With this invasion probably came also Man. It seems probable, therefore, that the earliest remains of man in America will be found on the Pacific coast.

Also the great Pliocene Lake, which stretched from near the shores of the Gulf far into British America, and possibly into Arctic regions, and formed a more or less complete barrier to the mammalian fauna east and west, *was abolished* by upheaval, and free communication was established. It is impossible that all these changes of climate and all these migrations, partly enforced by changes of climate and partly permitted by removal of barriers, and in this latter case especially attended with the fiercest struggle for life, should not produce rapid and profound changes in the mammalian fauna.

In Europe the process has been more accurately studied and is better known. In Quaternary times at least four different mammalian faunæ struggled for mastery on European soil. 1. The Pliocene Autochthones. 2. Invasions from Africa by opening of gateways through the Mediterranean—one by way of Italy, Sicily and Malta, and one by Gibraltar, both of which have been again closed. 3. Invasions from Asia, by removal of a great sea-barrier connecting the Black and Caspian with the Arctic Ocean. This gateway has remained open ever since. 4. Invasions from Arctic regions enforced by changes of climate. Probably more than one such invasion took place: certainly one occurred during the second Glacial epoch. The final result of all these climatic changes and these struggles for mastery was that the Pliocene Autocthones, adapted to a more genial climate, were mostly destroyed or else driven southward with some change into Africa; the African invaders were driven back also into Africa, and with some Pliocene Autocthones isolated there by subsidence in Mediterranean region closing the southern gateways, and still exist there under slightly modified forms; the Arctic invaders were again driven northward by return of more genial climate, and there remain to this day; while the Asiatics remained masters of the field, though greatly modified by the conflict. Or perhaps more accurately we might say that the existing European mammalian fauna is a resultant of all these factors, but the controling factor is the Asiatic. With the Asiatic invasion came man, and was a prime agent in determining the final result.

Thus, regarding the Tertiary and the Present as consecutive Eras and the Quaternary as the transition or critical period between; then, if the record of this period had been lost, corresponding with the unconformity here found, we should have had here an enormous and apparently sudden change of mammalian species. Yet this change of fauna, as great as it is, is not to be compared with that which occurred between the Archaean and Paleozoic, or between the Paleozoic and Mesozoic, or even that between the Mesozoic and Cenozoic; for the change during the Quaternary is mostly confined to species of the higher mammals, while the change during previous critical periods extended to species of all grades: and not only to species, but to genera, families, and even orders. We conclude, therefore, that *the previous critical periods or lost intervals were far longer than the whole Quaternary; or else that the rate of evolution was far more rapid in these earlier times.*

To sum up then in a few words the general formal laws of evolution-change throughout the whole history of the earth:

1. Gradual, very slow changes of form everywhere under the influence of all the factors of change, known and unknown—e. g., pressure of changing physical conditions whether *modifying the individual*, certainly one factor, or *selecting the fittest offspring*, certainly another factor; improvement of organs by use and the improvement inherited, certainly a third factor, and perhaps still other factors yet unknown. This general evolution by itself considered would produce similar changes everywhere, and therefore would produce geological faunæ, but not geographical diversity. Determination of a geological horizon would in this case be easy, because fossil species would be identical everywhere.

2. Changes in different places and under different physical conditions taking different directions and advancing at different rates, give rise to *geographical faunæ*. This, if there were nothing more, would produce far greater geographical diversity and more complete localization of faunæ and floræ than now exists; so great that the determination of a geological horizon would be impossible.

3. The forces of change resisted by heredity, in some species and genera more than others, determines paroxysms of more rapid movement of general evolution affecting sometimes species, sometimes genera or families. The sudden appearance of species, genera, families, etc., in quiet times is thus accounted for.

4. *During critical periods*, oscillations of the crust, with rapid changes of physical geography and climate, determine a more rapid rate of change in all forms, 1st, by *greater pressure of physical conditions*, and, 2d, by *migrations* partly enforced by

the changes of climate and partly permitted by removal of barriers, and the consequent *invasion* of one fauna and flora by another and *severe struggle* for mastery. This would tend to *equalize again* the extreme diversity caused by the second law; but the effect would be more marked in the case of animals than plants because voluntary migrations are possible only in this kingdom. Hence it follows that *a geological horizon is far better determined by the fauna than by the flora.*

III. *Historic value of the Present time.*—Most geologists regard the Present as one of the minor subdivisions of the Cenozoic Era, or even of the Quaternary period. More commonly the Quaternary and Present are united as one age—the age of man—of the Cenozoic Era. The Cenozoic is thus divided into two ages; the age of mammals commencing with the Tertiary, and the age of man commencing with the Quaternary; and the Quaternary subdivided into several epochs, the last of which is the Present or Recent. But if the views above expressed in regard to critical periods, be correct, then the Present ought not to be connected with the Quaternary as one age, nor even with the Cenozoic as one era, but is itself justly entitled to rank as one of the *primary divisions* of time, as one of the great eras separated like all the other eras by a critical period: less distinct it may be, at least as yet, in species than the others, the inaugurating change less profound, the interval less long, but dignified by the appearance of man as the dominant agent of change, and therefore well entitled to the name *Psychozoic* sometimes given it. The geological importance of the appearance of man is not due only or chiefly to his transcendent dignity, but to his importance as an agent which has already very greatly, and must hereafter still more profoundly, modify the whole fauna and flora of the earth. It is true that man first appeared in the Quaternary; but he had not yet established his supremacy; he was still fighting for mastery. With the establishment of his supremacy the reign of man commenced. An age is properly characterized by the *culmination*, not the first appearance, of a dominant class. As fishes existed before the age of fishes, reptiles before the age of reptiles, and mammals before the age of mammals. so man also appeared before the age of man.

We, therefore, regard the Cenozoic and Psychozoic as two consecutive eras, and the Quaternary as the critical, revolutionary or transitional period between. But since the record of this last critical period is not lost and we must place it somewhere, it seems best to place it with the Cenozoic Era and the mammalian age, and to commence the Psychozoic Era and age of man with the completed supremacy of man, i. e. with the Present epoch.

Berkeley, California, March 15, 1877.

ART. XVII.—*Notes on the internal and external structure of Paleozoic Crinoids;* by CHARLES WACHSMUTH.

THE structure of fossil Crinoids has occupied the attention of many able writers, and numerous ingenious and plausible theories have been advanced to demonstrate the physiological functions of the various parts of their complicated organization. The results of investigations heretofore made have been by no means harmonious, and newly discovered evidence renders many of these theories wholly unsatisfactory. I have been favored with unusual facilities for obtaining accurate knowledge upon many of the questions involved in these researches, and therefore hope that I may contribute useful information on the subject. The collections of eighteen years at Burlington, Iowa, have brought to light material, unrivaled elsewhere, for this study. I have obtained upwards of four hundred species of Crinoids at that locality, many of the specimens in such a condition, that not only the whole calcareous skeleton of the animal, but even the most delicate internal organs are preserved almost as perfectly as in those dredged from our present seas. Careful observations, extending through many years, and study of this material in connection with extensive collections from other formations, have enabled me to add to the present knowledge of these forms many interesting and important facts, and have led me to conclusions which I present in the following pages.

1. *The mouth and the tubular skeleton below the vault.*

The apparent absence of a mouth has proved to be one of the most perplexing points in the investigation of the structure of Paleozoic Crinoids. In all Radiates (even of the most inferior groups), this organ is located invariably *at one end of the vertical axis*, although that axis or center is not always the center of figure. It occupies in the recent Crinoids the upper end of this axis, but in many at least of the Paleozoic Crinoids, the portion of the summit, where from analogy, we should expect to find the oral aperture, is perfectly covered by solid and immovable plates. The only aperture in connection with the visceral cavity is lateral or subcentral, placed outside of the radiation and within the interradial area, where, from analogy, we must expect to find the anus. If, as Mr. Billings,* Dr. White† and the older writers on Crinoids supposed, this aperture served both as mouth and vent, so that these Crinoids took in their food through the anus, this stands as the sole

* This Journ., 1869, vol. xlviii, No 142, p. 69.
† Journ. Nat. Hist. Boston, 1862, vol. vii, No. 4, p. 481.

exception to the rule governing the class. It is true, the Ophiurans, for instance, have no separate anal opening, and the same aperture performs both oral and anal functions, but it is placed within the radial center and therefore cannot be homologized with the interradial orifice of Paleozoic Crinoids. In *Antedon rosaceus*, although the nascent Crinoid develops already within the pseudembryo a separate mouth and vent, a single orifice serves for some time both as oral and anal aperture, yet it is the permanent mouth occupying the center of the ambulacral system.* While we thus find the mouth performing permanently or temporarily anal functions, we have on the other hand no evidence either from recent nature or from embryology that an anus ever becomes developed into, or performs the office of, a mouth.

The Crinoids of our present seas live exclusively on microscopic food, and we must expect to find that the Paleozoic Crinoids subsisted upon very similar food and had a very similar mode of alimentation. Whenever in *Antedon* alimentary particles fall upon the furrows of the arms or pinnulæ, they are transmitted downward along these furrows to the mouth wherein the furrows terminate. Dr. Carpenter remarks on this subject:†

"The transmission of alimentary particles along the ambulacral furrows is the result of the action of cilia with which their surface is clothed. Although I have not myself succeeded in distinguishing cilia on the surface which forms the floor of these furrows, yet I have distinctly seen such a rapid passage of minute particles along their groove as I could not account for in any other mode, and I am therefore disposed to believe in their existence. Such a powerful indraught, moreover, must be produced, about the regions of the mouth, by the action of the large cilia which fringe various parts of the internal wall of the alimentary canal, as would materially aid in the transmission of minute particles along those portions of the ambulacral furrows which immediately lead toward it; and it is, I feel satisfied, by the conjoint agency of these two moving powers, that the alimentation of Antedon is ordinarily effected."

It appears from these observations, that the mouth of Antedon has no special functions as such, but is merely a receiving center or general passage, into which the food which accidentally falls in contact with the furrows of the arms or pinnulæ, enters; a passage which might as well be external, hidden beneath a vault, as open to the surrounding element, pro-

* Sir Wyville Thomson, Phil. Trans. of the Royal Society.

† Researches on the Structure, Physiology and Development of *Antedon rosaceus*. Part I, by W. B. Carpenter, M.D., F.R.S., Phil. Trans. Roy. Soc., vol. clvi, Part II, 1866.

vided the food could be brought in contact with it. The large cilia on the inner wall of the alimentary canal, which Dr. Carpenter describes as being capable of producing such a powerful indraught to the region of the mouth, afford, it seems to me, also a very satisfactory explanation of the mode by which the transmission of food was effected in Paleozoic Crinoids. How much more powerful must have been the effects of these cilia in individuals, in which mouth and furrow were arched over and in which the current was unobstructed from without. Considering, further, that probably the covered parts of the food channels themselves were fringed with cilia of similar functions, it could have been of but little moment how remote from the mouth the food entered. We find another most striking example in confirmation of this supposition in *Hypomene Sarsi* Lovèn, a recent Cystidian, indicating in analogy with recent nature that Crinoids had the mouth sometimes internal. Prof. Lovèn found in the covered parts of its channels microscopic Crustacea, larval bivalves, and other remains of the food of the animal, apparently taken through the open parts of the channels. Applying this observation to Paleozoic Crinoids it seems very probable that their food was taken up along the open parts of the arms or pinnulæ, and conveyed through the closed parts to the concealed mouth.

Dr. Ludvig Schultze, in his excellent "Monograph on the Echinoderms of the Eifel, Vienna, 1867," was, so far as I know, the first author, who suggested the idea, that all Crinoids which are covered on their ventral side by solid plates, and have but one orifice, were provided with an internal mouth. He further suggested, that the food was conveyed by the open food groove to the inner cavity through the arm-openings at the base of the arms, by means of subtegminal channels along the inner surface of the vault.

Dr. Lütken, fully confirming Dr. Schultze's observations, gives a full description of the ducts and subtegminal galleries, and compares these with the covered food grooves in *Hypomene Sarsi*, expressing the opinion, that the galleries underneath the summit, which he considered to be closed at the bottom and thus transformed into ducts, were food passages.

Meek & Worthen describe and figure in the Illinois Geological Report, vol. v, from my former collection, now in the Museum of Comparative Zoology at Cambridge, several specimens of well-preserved digestive organs, and also an *Actinocrinus proboscidialis*, in which a skeleton of tubular canals proceeds from a point below the central axis of the vault, to the arms. There are in that specimen five main tubes which bifurcate midway toward the arm bases, each division bifurcating again, sending a branch to each one of the twenty arms of that species. The

main tubes and branches are constructed on their lower side of alternating plates, upon which on either side, a second row of minute quadrangular interlocking plates is attached, longitudinally arranged, thus covering the tubes. The upper rows of plates are not preserved in this specimen, but I have found them in two specimens of *Strotocrinus*, which I obtained recently, in which they are well preserved and in place. The condition of the specimen, as Meek & Worthen remark, leaves but little doubt, but that the tubes form through the arm-openings of the calyx a continuation of the arm furrows. In removing parts of the vault, I unfortunately broke the upper part of the fragile skeleton, but enough is preserved to prove that the five main tubes did not connect directly with each other, but communicated at their upper end separately, as it seems, with an annular vessel of which traces are yet preserved. Such a vessel was found in wonderful preservation in a specimen of *Actinocrinus Verneuilianus* Shum. The radiating canals were here not preserved, but the little openings, through which they communicated, are plainly visible in the circular organ. There are, at the lower side of the ring, which is composed of minute interlocking plates, five other small openings, which, alternating with the former ones, were apparently in connection with organs of the interradial system (communicating perhaps with a circulatory system). The whole upper part of the stomach is here placed within the ring,—differing in this particular from *Actinocrinus proboscidialis*, in which only a spiral alimentary tube passes out from the interior of the convoluted digestive organ. The several tubes of the skeleton, though closely following the direction of the vault, but without touching it, are placed here within some obscure furrows along its inner surface.

Such furrows in the vault can be observed in many Paleozoic Crinoids. They are either elevations of the vault itself or are formed by ridges or partitions on the inner surface, which are always deepest toward the anal side. The grooves are sometimes closed underneath, particularly in very old specimens, thus forming regular ducts or tunnels. Their arrangement seems to be similar in all these Crinoids, no matter whether the species has a subcentral proboscis or merely a lateral opening, they always diverge from a plane on the inner wall of the vault, in front of the anus, and branch to the arm openings.

For further information on this subject, I will now call attention to some most excellent natural casts, mostly of Actinocrinidæ, which I obtained from cherty layers of the Upper Burlington limestone. The outer shell or limestone test was generally attached when I found them, but so much decayed,

that it was removed by the least touch. The substance of which the casts are formed appears to have been a fine silicious mud which could penetrate the smallest pores. The internal organs are of course not preserved, but their impressions at the surface of the casts throw much light on the structure beneath the vault. The center of radiation appears here a small pentagonal, rounded, or in species with strong subcentral proboscis, subtriangular or even heart-shaped space or plane, enclosed by a deep groove, from which, in some of these specimens, elevated ridges, alternating with depressions, pass out toward the arms; but before quite reaching them, there proceeds, from below the ridges of the casts to every arm, a smaller ridge which clearly indicates the tubular canal, as described in *Actinocrinus proboscidialis*. The casts are so perfect that I can even detect at some places the impressions of the alternating minute plates of the tubes.

The casts are easily understood, if we remember that the broader ridges are impressions of the grooves in the vault, and that the depressions correspond to the partitions which formed the grooves. The radiating tubes, where they do not appear in the casts, were evidently placed at some distance from the vault, and therefore enveloped and obliterated by the material forming the casts; but on approaching the arm-bases, they closely underlie the test, and their counterparts are preserved.

I have already mentioned in the casts a pentagonal space, surrounded by a furrow, as being the center of radiation. It is located anterior to the proboscis, occupying a central or nearly central position. The middle of the space is occupied frequently either by a small opening or by a little cone indicating an aperture leading toward the inner cavity, but in these casts the aperture is isolated, and there appears on the surface no connection with the annular groove surrounding it. To understand this structure it becomes necessary to examine first some other casts from the same locality, mostly of *Strotocrinus* and *Actinocrinus*, but also several of *Batocrinus*, though of different species from those which I have just described. These casts have no annular groove, and the radiation which is marked by elevated rounded ridges, almost like strings overlying the surface, proceeds from *a point in the center where I noticed the little aperture* in the former casts. The strings diverge toward the arm-bases in the same manner as the tubular canals; they are stronger toward the center, decreasing in size with each bifurcation. That these ridges are remains of muscular cords is not probable from the perishable nature of such organs, and they are not their casts, or they should have left depressions in place of elevations. They can only be casts of passages which communicated with the central aperture, and which were evidently

yet preserved when the siliceous mud, forming the casts, penetrated the body, but, their calcareous parts becoming in the course of time decomposed, a cast was left only of their inner channel, and this explains their string-like appearance. The little central aperture, located at the upper end of the vertical axis, occupied on the casts, and hence below the vault of these Crinoids, exactly the same position that *the internal mouth of Antedon occupies at the peristome*, while the position of the string-like ridges (in case they represent passages as I can hardly doubt), is analogous with that of the *open food grooves* of recent Crinoids.

The annular groove on the casts is probably an impression of the annular *vessel* of which the calcareous parts have decomposed. This organ, in the fossil state, heretofore only observed in the case of *Actinocrinus Verneuilianus*, existed undoubtedly in all Crinoids. That we find no trace of it in some of the casts is no proof to the contrary; it may have been sometimes composed of more perishable material and was not preserved, or situated at a greater distance from the vault and covered by the substance of which the casts were formed.

2. *The ventral furrow of the arms.*

The arms of Palæozoic Crinoids manifest great diversity in outer form and structure, but are invariably provided with a ventral furrow, which continues from the arm bases up to the tips of the arms and along the pinnulæ. The pinnulæ spring out alternately right and left from the arm-plates, their furrows connecting with that of the arm and forming in fact a continuation of the same.

The furrow appears, in specimens in ordinary preservation, as a simple groove, which communicates through the arm openings with the inner cavity of the body. Only in rare instances, has the furrow been found covered by minute plates, whose construction however has heretofore not been ascertained. The best specimen of this kind, that I have seen, is a *Cyathocrinus malvaceus*, in which the little plates above the furrow can be studied in all their details, with the greatest precision. The specimen is the property of Frank Springer, Esq., who had the kindness to leave it with me for investigation and description. The arms of Cyathocrinus are composed of long slender joints with a wide ventral furrow. They bifurcate frequently, each branch bifurcating at intervals again. There appears on the arms of *Cyathocrinus* no scar for the attachment of pinnulæ, and as these appendages have never been observed in the genus, it is probable that the many little branches performed their functions. In Mr. Springer's specimen the plates above the furrow consist of two rows of minute pieces, on

either side, the inner rows of which join in the middle, interlock with each other, and form an apparently solid covering. The outer plates which are attached to the arm-joints are toward the upper end of the arms placed partly upon the edges of the joint, but nearer the calyx rest wholly against the edges of the upper part of the ventral furrow. They are longitudinally arranged, partly hidden from view by the inner plates. The visible part is quadrangular, with a narrow tooth-like projection toward the lower end of each plate, which is directed inward and slightly downward as a sharp, elongated process, and forms a support for the inner plates. The inner plates are elongated triangular, resting with their shorter sides against the inner faces of the outer series, and, between the tooth-like extensions, overlapping them with their beveled lateral edges, in such a manner that each plate exteriorly fits in and fills the space between each pair of similar triangular plates on the other side. The two longer sides interlock with corresponding sides of similar plates of the opposite row, their sharp angles or apices meeting the sutures between the opposite quadrangular plates. At each of these points of juncture, just beyond the apex of each triangular piece, on either side of the furrow, there is a little pore, which evidently communicates with the inner channel. There are six sets of plates to each arm-joint, all the plates being imbricated from the lower side upward,—that is, the lower ones lap slightly the edges of the upper ones —thus facilitating the movements of the arms.

In describing the skeleton below the vault, I suggested that the tubes were a continuation of the arm furrows. A transverse section of the arm, examined with the aid of a good magnifier, shows that the tubes themselves were, at least in this species, continued along the arms to their tips, and rested within the arm-furrow, with their sides closely attached to the upper edges of the arm-joint. The tubes do not touch the floor of the arm-grooves, but leave a good sized subtriangular channel underneath. The small plates, above described, form the upper or outer wall of the tube, and two sets of small plates enclose it below. The position of the two upper sets of quadrangular pieces is nearly erect, leaning inward, the triangular cross-pieces lying horizontally, thus forming a regular tube or tunnel with a nearly round channel.

I was at first of the opinion, after examining Mr. Springer's specimen, that the arm-furrows of Paleozoic Crinoids were permanently covered by solid plates, like the ventral side of their cup, but upon comparing these arms with those of a specimen of *Cyathocrinus viminalis* in my own collection, I became fully convinced, that the inner plates could be opened or shut at will by the animal. The arms of my specimen are spread

out, their ventral furrows are open, the quadrangular pieces in place, their tooth-like extensions stand out like the teeth of a saw, and are so arranged that the indentations face the salient angles of the opposite side, thus giving to the furrow a strongly zigzag appearance.

The tooth-like processes in this species are so prominent, that from their similar form, and before I had recognized this peculiar structure in Mr. Springer's specimen also, I at first took them for the triangular cross-pieces, but on closer inspection I found no sutures between the projections and the quadrangular portions of the plates; and as the place of attachment for the triangular pieces is plainly visible, there can be no doubt but that these plates were not preserved in the specimen. Furthermore, as the quadrangular pieces are with slight interruptions found on all the arms of the specimens most beautifully preserved, it seems almost impossible to understand how the triangular pieces could have fallen out, if they had been fastened solidly to the adjoining plates. Supported by the tooth-like projections, and resting against the edges of the quadrangular plates, they were evidently better protected than the outer pieces, and it seems to me their destruction would have involved that of the entire covering. I therefore believe these plates were not firmly attached in the living animal, but merely leaned against each other as well as upon the tooth-like projections, being only attached to the inner edge of the quadrangular plates by muscular or interarticular substance, and that they were, in analogy with similar plates in recent Crinoids, movable. This seems further confirmed by the construction of the plates themselves, and especially by the manner of their attachment. The inner edges of the quadrangular plates (between the projections), being slightly convex, they rested in regular sockets which facilitated their opening in an outward direction. In case these plates, as I can no longer doubt, could be opened or closed, it seems reasonable, that they were open in my specimen when the animal died, or they otherwise would have been preserved.

The position and construction of the inner channel proves most satisfactorily and in analogy with recent Crinoids, that it contained the *food-groove* which conveyed the food through the arm-openings beneath the vault to the oral aperture. The small movable plates are evidently homologous with the "*saumplatten*" of Antedon, and the imbrications of these plates, as well as of the entire covering, seems to hint at the conclusion, that the furrow was always closed when the arms were folded up as in Mr. Springer's specimen; but that on the contrary, as in my specimen, the furrow was open when the arms were spread, and that in this position the animal took in its food.

In describing the covering of the furrow, I have already mentioned the presence of two rows of small pores located at the angles of the triangular pieces. There is nothing to indicate that these pores were sockets of pinnulæ, and if they were, the "saumplatten" could not have opened. From their position I infer they were passages for tentacles connecting with parts of the inner tube. If this is correct, it seems to me, there must have been located within the tube a passage in connection with the ambulacral system, since the tentacles form a part of it. This is evidently the case. In a transverse section of the arm, with the help of a magnifier, I think I have detected within the tube, traces of two passages: *a deep groove* occupying only the median region, and on each side of it *a small canal*, underlying the pores. The condition of the specimen does not enable me to say whether the two side passages connect at the bottom or not, but in either case, they undoubtedly represent *the ambulacral canal*, the food-groove occupying only the median and upper part of the channel.

It is to be regretted that in no instance the upper part of the tubular skeleton has been found in perfect preservation. There has been observed beneath the vault an annular vessel, constructed of plates similar to those of the radiating tubes, with small openings directed toward the radial sides of the specimen, with the alimentary canal passing through the inner space of the ring, but its connection with the surrounding parts was not preserved. The position of the annular organ in the center of radiation leaves but little doubt that it is *the œsophageal ring* or *center of the ambulacral system.* The great similarity in the construction of its plates, the presence of openings corresponding to the direction of the tubes, indicates most strongly that the tubes and the circular organ were connected, and that the ambulacral canal, which I recognized in the lower passage of the arms, communicated with the pores. But, as seen from the casts, there proceeded below the vault from the arm-bases to the center, another series of passages, which passing the region of the annular vessel, unite in the center. I hold these to be a continuation of the food-grooves in the arms, which evidently passing over the top of the circular organ terminated within the central orifice.

The position of the œsophageal ring seems at first sight to be not quite in harmony with recent nature; this organ, which in all Echinoderms with an external mouth, is attached to the inner side of the test, is located in Paleozoic Crinoids at a distance from the vault. However, considering that the aboral vault cannot be homologized with the oral skin of recent Crinoids, and that only the tubular skeleton corresponds to the radiating passages connecting with the peristome, the vault

thus forming a mere covering, we shall find the position of the circular organ perfectly harmonious with that of all other Echinoderms.

I have mentioned already that there exists in the arm-groove of *Cyathocrinus*, beneath the tube and at the bottom of the furrow, another canal, as large or larger than the channel of the tube, whose functions are unknown. It was, apparently, through the bottom of the arm-openings, connected with the perivisceral cavity of the calyx and may have served for several offices, containing perhaps also the cœliac canal.

Dr. Schultze, in his "Monograph on the Echinoderms of the Eifel," p. 17, gives a most excellent description of the arm-furrow in *Cupressocrinus*. He found two sets of plates covering the furrow like a roof, and asserts, that the inner pieces could be turned back in the living animal. I had overlooked this in making out my descriptions, but it was pleasing and highly satisfactory to me to find that we both had arrived independently at the same conclusion. The construction of the arm-furrow of *Cupressocrinus* is very similar to *Cyathocrinus*. In a section of the arm of this genus, I readily distinguished, by transmitted light, the food-groove, which has a narrow and deep outline, a canal on both sides of it, and I have but little doubt but that the arm-furrows were similarly constructed in all Palæozoic Crinoids.

3. *The alimentary canal.*

Meek and Worthen publish in the Geological Report of Illinois, vol. v, most excellent descriptions and figures of an organ which occupies the greater part of the visceral cavity of Paleozoic Crinoids; they call it from its position in analogy with other Echinoderms "*the digestive organ.*" It is a large convoluted body, resembling in outer form and outline the shell of a Bulla, with a longer vertical axis, and open at both ends. The upper end is placed below the center of the ventral disc, the lower one directed toward the column, dilated above, contracted below, coinciding with the inner space of the visceral cavity to the walls of which it stands nearly parallel. In some cases it is subcylindrical and slightly truncate at both ends.

The organ is constructed of a great number of very minute pieces or bars with intervening meshes, but its delicate texture is but seldom observed, owing to the presence of incrustations of calcareous or siliceous matter which fill up the meshes, and give to the structure a rather dense appearance.*

* To these incrustations which are evidently deposits from the water, we owe in a great measure the preservation of these delicate organs, and as they are comparatively thicker in adult specimens, they seem to have accumulated already during the life of the Crinoids, and may have caused in many instances their death.

The convolutions are directed outward from left to right, varying in number from two to four in different species. Judging from external appearance only, the convoluted walls of the organ appear as mere partitions leading to the inner chamber of a bulla-shaped body. This however is not the fact. Examining the so-called walls in some transverse sections, I find them to be coiled, without touching each other at any point, and composed of two distinct partitions, placed side by side and closed at the edges, thus proving that the apparent walls are the coiled organ itself. According to this, the digestive organ consisted of a long flattened canal, rounded at the outer side, widest in the middle, tapering rapidly at both ends to a rather heavy flattened tube, the outer end ascending spirally toward the top of the visceral cavity, making two or more turns, while the inner one, winding in a spiral way around its own axis, passed upward to near the center of the dome.

In a specimen of *Actinocrinus* in which the digestive organ is apparently perfect, though showing the usual rough appearance, I succeeded in removing at one side the two upper convolutions, in such a manner that the detached parts can be replaced or lifted up for investigation. I had here an opportunity to examine the inner or more properly upper end of the alimentary canal (as distinguished from the outer end or terminal part). The top is unfortunately hidden below some inorganic matter, but enough can be seen to prove that it proceeded evidently from a place below the center of the dome. The organ, where it comes into view, is an elongated tube, which passing downward, widens first gradually to near the middle of the visceral cavity, then rapidly until it acquires the width of at least two-thirds of the entire length of the cavity. The upper end in descending spirally turns from right to left, but on becoming wider curves sharply in the opposite direction and the convolutions are now directed from left to right. The outer end also tapering rapidly and forming a flattened tube, ascends the outside spirally from below all the way up to the top, and I am inclined to suppose, proceeded to the proboscis, being probably analogous with the terminal intestine of the Echini, while the upper end communicated with the food-groove.

Such, with slight modifications, was probably the construction of the alimentary canal of all *Actinocrinidæ*, *Platycrinidæ*, etc., but not that of the genus *Ollacrinus*.* I found the alimentary

* Figured by Cumberland without generic or specific diagnosis or specific name, London, 1826, in the Appendix to Reliquiæ Conservatæ.

Synon.—*Gilbertsocrinus* Phillips, 1836, Geol. Yorkshire, Pl. II, p. 207. *Goniasteroidocrinus* Lyon & Casseday, Suppl. Geol. Rept., Iowa, p. 70. *Trematrocrinus* Hall, 1860, Suppl. Iowa Geol. Rept., p. 70.

As Cumberland's figure is perfectly correct and easily identified as *Ollacrinus* (*Gilbertsocrinus*) *calcaratus*, his generic name "*Ollacrinus*" must be retained according to the laws of nomenclature.

canal partly preserved in *Ollacrinus tuberculosus* Hall, in which it seems to have been composed of the same delicate network, but the organ consists here of a round canal which descends spirally, and contracting gradually, takes at the lower portion of the visceral cavity an upward direction. The upper part of the organ is unfortunately not preserved in this specimen.

4. *The anal aperture and the proboscis.*

The anus of Paleozoic Crinoids is placed always within one of the interradial series, which is generally wider than—and often constructed differently from—the others. The aperture is situated either in some part of the calyx itself, or at the top of a long proboscis. It is a most remarkable fact that genera which evidently belong to the same group, even species, apparently of the same genus, for instance *Strotocrinus*, differ so widely in the construction of this organ. Some having a long massive tube, reaching to several inches above the tips of their arms, while others are provided only with a plain lateral opening without any superstructure whatever.

I do not speak at present of the inflated or balloon-shaped proboscis of *Zeacrinus*, *Cœliacrinus*, *Poteriocrinus*, *Heterocrinus* and similar genera, in which this part is more properly called "*the ventral sac*," as it evidently formed a large portion of the visceral cavity. Its great size compared with the lower cup, the presence of large numbers of small pores, and the position of the anal aperture near the bottom instead of at the summit, seems to imply that the anal apparatus occupied in the internal economy of this sac only a limited space. The inflated sac can accordingly not be homologized with the slender, heavy plated tube of *Actinocrinus*. We can only compare its lateral opening, which is generally placed low down near the arm-bases, with the anal aperture of species in which the anus is located in the ventral disc.

In addition to its regular functions, the proboscis of Palæozoic Crinoids may have had the office of expelling the water from the system. This suggestion looks not unreasonable, if we consider that the solid body of the majority of these Crinoids had apparently no other outlet. I found in one instance the proboscis split open longitudinally, and within its inner cavity a well defined narrow tube, filling scarcely one-fourth of the inner space. This tube may have connected with the terminal intestine which I have described above, and the office of the surrounding canal may have been to eject the deoxygenated water from the body. The fact that some Crinoids were provided with a proboscis reaching beyond the region of the arms, and others with no proboscis whatever but simply a lateral opening, is easily explained, for if the rejected

matter were emptied between the arms, it must have come constantly again in contact with the arm currents, which is obviated by either plan. This accounts also for the fact that the proboscis of some species of *Eretmocrinus* is constantly turned to one side. The proboscis formed a natural support for the slender arms, for they are found in most specimens leaning closely against it, while in *Dorycrinus* which has no proboscis, the arms appear always clinging to its long, heavy spines, which are evidently not weapons of defence, as some authors have supposed, but merely a support and protection for the arms.

Dr. C. A. White describes in the Boston Journ. of Nat. Hist., vol. vii, No. 4, p. 489, several specimens in which the proboscis diverges at some distance above the ventral disc into two distinct branches. This may be in some instances the result of accidental development, but is more frequently due to an obstruction of the anal canal. I found a specimen of *Batocrinus longirostris* in which close to the vault the proboscis branches into two equally heavy tubes, and there appears immediately above their junction a strong inflation or kind of abscess. In another specimen, a stoppage or disconnection must have occurred within the body, for a second proboscis was formed at the ventral disc, developed here, as in every other instance, within the anal series or posterior side of the Crinoid. In one remarkable specimen a second proboscis breaks forth even at the lower end of the calyx, just above the basal plates. The pressure against these parts must have been enormous, for it caused the destruction of an entire ray, the plates of which are bulging out, forming together with the anal plates, and intermingled with smaller plates, such as ordinarily compose the proboscis of this species, a large elongated cavity with a rather large aperture. All these instances give evidence of a pressure from within, and indicate that the outside opening of Paleozoic Crinoids was solely an ejective organ, and could not have had oral functions. I have already mentioned that the anus is separated from the radial series by deep partitions at the inner surface of the vault, thus excluding any connection with the upper end of the digestive organ. Moreover, the casts of *Actinocrinus* show that the course of the proboscis is directed toward the posterior side, and the development of the abnormal proboscis occurs invariably in the anal series. It is therefore hardly necessary to argue on Dr. White's supposition that the abnormal second proboscis, wherever it occurs, might have served as buccal orifice, as such a theory is unsupported by analogy.

(To be continued.)

ART. XVIII.—*Chemical constitution of Hatchettolite and Samarskite from Mitchell County, North Carolina;* by OSCAR D. ALLEN, Professor of Analytical Chemistry, Sheffield Scientific School. *Contributions from the Sheffield Laboratory.* No. XLVIII.

I. *Hatchettolite.*

IN an article on the Samarskite of Mitchell County, North Carolina, (this Journal, March, 1876), E. S. Dana described a mineral associated with it as possessing the following properties: Specific gravity, 4·794, crystalline form, regular octahedrons with cubical faces, also the planes 3-3, color yellowish brown, luster resinous, blowpipe characters agreeing closely with pyrochlore. At that time I was able to make only a partial qualitative analysis of this mineral with ·25 gram placed at my disposal by E. S. Dana. The result showed the presence of lime, uranium, iron, a trace of magnesia, tantalic and columbic acid, and probably titanic acid.

In an elaborate paper on American columbic acid minerals, published in this Journal (May, 1877), Professor J. Lawrence Smith has also given a description, accompanied by analyses, of this mineral, for which he has proposed the name Hatchettolite.

Having recently obtained from Mr. L. Stadtmüller a specimen of this new mineral, which he received in a lot of Samarskite sent to him from the locality above mentioned, I have analyzed it, hoping to gain some additional facts bearing on its chemical constitution. The specimen was a large, but imperfect crystal. It was broken into small pieces, and afforded twelve grams of material judged sufficiently pure for analysis. Pieces from different parts of the mass gave the specific gravities, 4·77, 4·84, 4·82, 4·90, 4·76. The results obtained are here presented, Nos. I and II, together with one of the analyses of Dr. Smith, No. III.

	(I.)	(II.)	(III.)
Tantalic acid	29·83	29·60	
Columbic acid	34·24	35·94	67·86
Titanic acid	1·61		
Tungstic and stannic acids	·30		·60
Uranium oxide	15·50		15·63
Lime	8·87	8·89	7·09
Yttria and cerium oxides			·86
Iron protoxide	2·19	2·33	2·51
Magnesia	·15		
Potash	*trace**		1·21
Soda	1·37		
Water lost by heat	4·49		4·42
Lead	*trace*		*trace*
	98·55		100·18

* Not separated from soda.

A comparison of these numbers leaves no room to doubt that the same mineral was analyzed by Smith and myself. The sum of metallic acids in No. 1 being 65·68, in No. 2, 65·54, in No. 3 (of Smith) 67·86.* For the separation of tantalic and columbic acids the method of Marignac was used, which may be briefly described as follows. The mixed acids, after weighing, are fused with $HKSO_4$, washed with hot water to remove most of the H_2SO_4, dissolved in hot HFl. One-fourth their weight of $HKFl_2$ is then added. If crystals do not separate on cooling, concentrate to 7 c. c. per gram (of mixed acids) and let cool; K_2TaFl_7 then separates if tantalum is present in the solution. This salt is collected on a weighed filter, washed with cold water till the washings give no longer an orange-colored precipitate with solution of galls. Dry at 100° and weigh; continue the same process with the filtrate and washings, adding new portions of $HKFl_2$. One readily sees when the acicular crystals of K_2TaFl_7 mix themselves with the lamellar crystals of K_2CbOFl_5. Then redissolve the latter and collect the former on a filter. If the weight of both acids is known, Cb_2O_5 may be calculated by difference. On account of the suspected presence of titanic acid in the substance here examined the second crop of crystals of K_2TaFl_7 was redissolved in hot water and recrystallized. No more acicular crystals could be obtained in sufficient quantity to collect. For the same reason the bisulphate fusion of the mixed acids was treated with *cold* water. The filtrate and washing on boiling deposited a light precipitate, which brought into solution and tested with tin gave an unmistakable reaction for titanium. Moreover, after separation of tantalum the columbic and titanic acids remaining in solution were obtained by evaporating with H_2SO_4 and boiling with water and fused with K_2CO_3. Treatment of the fused mass with water left but very little residue, which further examination proved to be mostly potassium titanate.

In analysis No. 1, the two products containing titanic acid, obtained in this way, were fused with $HKSO_4$ which rendered the greater part soluble in cold water. This solution on boiling deposited what was assumed to be titanic acid, after weighing and subjecting to the further tests. It can hardly be expected that a very accurate separation was effected. By boiling ·5 gram of the mineral with H_2SO_4 mixed with a little HFl in a platinum crucible so rapidly as to exclude air decomposition was effected in a few moments. The diluted solution deoxydized an amount of permanganate of potash corresponding in one experiment to 1·90 per cent FeO, in another to 2·20

* Professor Smith remarks, "I refer to the metallic acids in the analyses in this paper as columbic acid from the fact that the tantalic acid which accompanies it is only in small quantities, and also because none of the methods proposed for separating the two acids have given satisfactory results in my hands."

per cent. Iron was therefore assumed to exist as protoxide and uranium as UO_2O. A calculation of the relative number of atoms from No. 1 gives the following:

Atomic weights.			
182	Ta	·134	·386
95	Cb	·252	
50	Ti	·020	
U 240	UO_2	·054	·268
	Fe	·030	
	Mg	·004	
	Ca	·158	
	Na_2	·022	
	H_2O		·255
	Sn?	·002	

Disregarding the small amount of tin or tungsten and deducting Ti with $\overset{II}{R}$ sufficient to form with it normal titanate, there remain $\overset{II}{R}$ ·248 $\overset{V}{R}$ ·386 H_2O ·255, whence $\overset{II}{R}:\overset{V}{R}:H_2O=2:3{\cdot}1:2$, a ratio closely approximating to that required by $\overset{II}{R}_2\overset{V}{R}_3 2H_2O$ or $\overset{II}{R}_2\overset{V}{R}_2O_7+2\overset{II}{R}\overset{V}{R}_2O_6+4H_2O$, in which $\overset{II}{R}$ represents one atom of a bivalent basic radical or two of sodium and $\overset{V}{R}$, Ta, or Cb. The investigations of Rammelsberg* lead to the conclusions that three columbates (in which Cb may be replaced by Ta) occur in minerals, viz: $\overset{II}{R}Cb_2O_6$, $\overset{II}{R}_2Cb_2O_7$, and $\overset{II}{R}_3Cb_2O_8$, corresponding to phosphates and arsenates; which, singly or combined with each other, with or without titanates., zirconates, thorates, etc., constitute mineral species. Hatchettolite appears to be composed of the first two with a small quantity of normal titanate.† It it perhaps questionable whether there is any significance in the fact that the amount of water present bears a simple ratio to the other constituents.

Some pieces from one side of the specimen from which material for analysis was selected had the specific gravity from 4·60 to 4·70 per cent and were rejected. They lost by ignition 4·97 per cent. Hatchettolite may have resulted from alteration of a mineral having essentially the same chemical constitution, as well as crystalline form, as pyrochlore, an alteration consisting of hydration and removal of alkaline fluorides.

II. *Samarskite.*

Samarskite from Mitchell Co., according to the analysis of the writer,‡ has the following composition:

* Jour. Chem. Soc., vol. xxv, p. 189.

† Rammelsberg (Jour. Chem. Soc., vol. xxv, p. 203) supposes that these three compounds may be isomorphous, and that they exist in the isometric crystalline form in pyrochlore.

‡ Dana's Text Book of Mineralogy, p. 340, March, 1877.

		(1)		(2)	Atomic ratio.	
Columbic acid	55·60	37·81	55·80	37·20	·2754	·3592
Tantalic acid		17·79		18·60	·0838	
Stannic acid		----		0·08	·0005	·4430
Yttria		14·52		14·45	·1896	
Cerium oxides*		4·10		4·24	·0382	
Uranium oxide (VO_2O)		12·63		12·46	·0433	
Manganese protoxide		0·80		0·75	·0106	
Iron protoxide		10·60		10·90	·1513	
Lime		----		0·55	·0100	
Water		----		1·12		
				100·35		

These results do not differ materially from the first published analysis of samarskite from the same locality by Miss Ellen H. Swallow, who found 54·96† per cent of metallic acids of the tantalic group, and expressed a wish that some one might undertake their separation‡ In my analyses, tantalum and columbium were separated by Marignac's method. Water expelled by ignition was collected by a calcium chloride tube. 0·5 gram of the very finely pulverized mineral was completely decomposed by heating with sulphuric acid in a sealed glass tube. The solution, properly diluted, decolorized an amount of potassium permanganate corresponding to 11 per cent of protoxide of iron. A repetition of this experiment confirmed the first result. It may therefore be concluded that no material amount of uranium is present in the lower state of oxidation (UO_2). The relative number of atoms of each element present is calculated from No. 2, uranium being assumed to exist as UO_3 or an oxide (UO_2O) of the bivalent radical UO_2 which it is reasonable to suppose acts as a basic radical with tantalic and columbic acids, as it does with phosphoric and arsenic acids in several natural compounds. $\overset{II}{R} : \overset{V}{R}$ is then 4430 : 3592 = 1·236 : 1 = 4·94 : 4 closely approximating 5 : 4 required by the formula $\overset{II}{R}_2\overset{V}{R}_2O_7 + \overset{II}{R}_3\overset{V}{R}_2O_8$.

In chemical constitution this samarskite appears to approach Fergusonite being next to this mineral the most basic of the natural columbates and tantalates. The formula of Fergusonite, deduced by Rammelsberg from a large number of analyses, is $\overset{II}{R}_3(Cb, Ta)_2O_8$.

* The atomic weight of didymium was used in calculating the atomic ratio. This element is readily shown by spectroscopic examination of a solution. Only a minute quantity of cerium is present, the presence or absence of other elements of the cerium group was not ascertained.

† Dr. Smith's analysis, contained in the paper already cited, gave 55·13 columbic acid.

‡ Proceedings of the Boston Society of Natural History, vol., xvii, 424, 1875. See also this volume, p. 71.

ART. XIX.—*On the relations of the Geology of Vermont to that of Berkshire;* by JAMES D. DANA.

[Continued from page 48.]

IN the preceding part of this paper, the unity of the system of rocks occupying the limestone region of the Green Mountains has been demonstrated by reference to facts connected with the geographical arrangement and stratigraphical relations of the formations. One other well-known feature characterizes the belt from north to south, and requires consideration: namely—

3. THE ABUNDANT OCCURRENCE OF IRON.

The existence of iron in the rocks is known mainly from the beds of limonite along the region. This limonite, as is now well known, was made from the rocks *in place*, and hence it is testimony as to what the original rocks contained and still contain where unaltered. Its beds exist in Vermont at so many places that the geologists of the Vermont Survey represented it on their geological map of the State as a continuous band of limonite. And south of Vermont it occurs more or less in each of the towns over the limestone area, and in some of them in deposits of great depth, the most noted being in Richmond and West Stockbridge, Massachusetts; Salisbury and Sharon, in Connecticut; Hillsdale, Copake, Ancram, North East (near Millerton), Amenia, Dover, and Pawling, in New York. In order that this feature of the region may be appreciated, I mention here the more prominent facts connected with the ore-beds. In a general way, and for special localities, the subject has been often treated. The descriptions given in this place have particular reference to the regional characteristics of the beds in the limestone belt, as illustrated by their mineral nature, distribution, and stratigraphical relations; and they have been drawn mainly from my own observations.

(1.) *The mineral nature of the beds.*

Throughout the great limestone area, from north to south, the ore-beds are alike in the nature of the ore, its mode of aggregation, and its association with yellow ochre and clays. Moreover, in most of the beds to the north as well as south more or less manganese is present with the iron. Manganese ore (chiefly pyrolusite and psilomelane) has been observed in connection with ore-beds of the Vermont limestone area in Bennington, Chittenden, Pittsford, Brandon, Middlebury, Bristol; and the Vermont Report adds, "in the vicinity of most of the beds of limestone in the State." It has been observed also in various ore-beds of Berkshire and New York, though less

abundantly than in Vermont. An analysis of ore from the Leet ore-bed, West Stockbridge, by Dr. A. A. Hayes, made in 1845, obtained red oxide of iron 76·18, oxide of manganese 5·04, phosphoric acid 2·36, water 10·80, quartz and gangue 3·40 = 98·78.* At the same time, phosphoric acid is very sparingly present; and there is only a trace of sulphur or none at all. The clay, while generally impure from the presence of iron ochre and other mineral material, is sometimes a white kaolin.†

(2.) *The geological distribution of the beds.*

(1.) The limonite deposits are alike throughout the limestone area in their interrupted occurrence and varying depths—even many miles often intervening between those of workable value, and all depths existing from zero to one hundred and fifty feet or more.

(2.) The ore-beds occur in the vicinity of all the rocks of the limestone area,—the limestone, clay-slate, hydromica slate, mica schist, gneiss and quartzyte. But, from north to south along the limestone area, they most abound where the schist is the hydromica slate, and hardly at all where it is common argillyte. The hydromica slate in Vermont prevails over the eastern half of the area, and, in Berkshire and farther south, over the western half; and accordingly the line of maximum ore-beds does not follow the general course of the limestone area, but is a little oblique to it, having more westing to the south. In Vermont it is with few exceptions not far from the western border of the eastern quartzyte range, as we learn from the Vermont geological map; the few exceptions occur near the center of the area, adjoining small central slate ridges, as in Dorset and on the borders of Wallingford and Tinmouth; and at the south end of the State in Bennington. These last are near the Taconic (or Great Central) slate-belt.

In Berkshire, beds exist sparingly in the eastern portion of the limestone area (as in the gneiss and mica schist region of Tyringham); the chief beds are in its *western* half in Richmond and West Stockbridge, a few miles east of the Taconic range. In Connecticut, they closely adjoin the Taconic range in Salisbury and Sharon; while in New York, they occur on

* I am indebted for a copy of this analysis to Mr. J. W. Hoysradt, President of the Company. Mr. Hoysradt states that no later analysis has been made; but he is of the impression that the ore now obtained yields less manganese and phosphoric acid.

† While limonite (in which the oxide of iron, Fe_2O_3, and the elements of water are in the proportion of 2 to 3) is the principal ore, there is also at Salisbury, as first shown by Prof. G. J. Brush (this Journ., II, xliv, 219, 1867), and probably also at other places, the related ore *turgite* (in which the same ratio is 2 : 1), and a little göthite (in which the ratio is 1 : 1); also occasionally a red ochre, which may be either turgite, or the simple anhydrous oxide Fe_2O_3. Besides the iron and manganese ores, there are also traces of cobalt and zinc.

the *west* of the Taconic range, in the towns of Hillsdale, Copake, and North East, besides along its eastern border in Dover, Amenia and Pawling. The axis of the band of maximum ore-beds appears hence to cross the axis of the limestone area in Bennington, Vermont; and to strike the Taconic range south of West Stockbridge, from which point, beds occur on both sides of the range.

(3.) *The stratigraphical relations of the beds.*

The ore-beds, to the north as well as south, occur near the junction of a stratum of limestone with one of hydromica or mica schist, the beds of both having usually a high dip (generally between 40° and 55°); or they have schist on both sides; and sometimes they are cut through by one or two beds of limestone or schist. Quartzyte often lies along the east side in Vermont; but a stratum of schist usually, if not always, intervenes between the quartzyte and limestone.

This geological relation of the beds was recognized, at localities in Vermont, Berkshire, and Connecticut, more than fifty years since, by the late Prof. Chester Dewey, then Professor of Geology and Mineralogy in Williams College, Williamstown. In the fifth volume of this Journal, 1822, in an article on the ore-bed of Bennington, he says (p. 251)—"It has been remarked that the great bed of ore is not *immediately* connected with any rocks. It seems, however, to be associated with limestone rocks, and the whole to lie between two strata of mica slate. It lies in the same [mountain] range with the ore of Salisbury, Connecticut, and has the same range of mica slate lying on both sides of it." The *in situ* position of the ore-beds is here brought out by Dr. Dewey, and a relation to the limestone is suggested. Two years later, in vol. viii, speaking of the ore-beds of Berkshire, he says (p. 30), that the beds "are near limestone, but on beds of clay." And "as mica-slate is found on both sides of them, they must doubtless be considered as lying in this rock, though the clay indicates that they are a later deposit than the rock itself:" in which the limonite beds are made a part of the mica slate, but not an original part.

The facts at many of the ore-pits of Berkshire and New York, as well as Connecticut, sustain the statements of Professor Dewey. The schist often forms part of the walls of the ore-pits, or stands in ledges near by; and when no rock is to be seen, the clays often show that they are the decomposed schist *in place* by their having its schistose structure, its dip and strike, and also its flexures, all corresponding precisely with the stratification of the rock of the region. The schist makes sometimes the eastern wall, and sometimes the western; and it is also found dividing the ore of the beds. Among the

New York ore-beds the relation of the schist is well exhibited in the Weed ore-pit in the town of Copake, and in the ore-pit, a mile and a half to the west of the village of Pawling.

Limestone strata outcrop near or within many of the pits. In Richmond, Mass., at the Cone ore-pit, a steeply dipping limestone stratum forms part of the northwest side; and at the Cheever ore-pit there is an outcropping bed a few yards to the west, conformable with the slate; at the Leet ore-pit, in West Stockbridge, limestone forms a ledge close by the pit. At the Miles ore-pit, in Copake, a bed of limestone was met with cutting through it, having the strike and dip of the region. The Vermont Geological Report states (p. 820) that a limonite bed in Western Bennington reposes on an impure ferruginous limestone, and it alludes to the similar position of the ore in other beds of the State.

(4.) *The source of the ores and clays.*

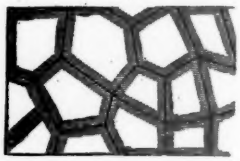
Carbonate of iron (spathic iron, or siderite of mineralogy) occurs in masses, in connection with some of the ore-beds; and part of the limestone is ferriferous, that is, either a carbonate of calcium-and-iron, or else, a limestone containing more or less of carbonate of calcium-and-iron. And in consequence of the extremely easy oxidation of the iron of these carbonates, such beds, when air and moisture have access to them, are in all cases undergoing alteration to limonite. In some of the ferriferous limestone manganese replaces part of the iron.

The pure carbonate of iron has been found thus far only in western Berkshire. At the Leet ore-bed, one and a half miles west of the village of West Stockbridge, there projects from the east side of the pit, what is called the "white horse," which is a large ledge of compact light gray spathic iron, of a grayish white color, distinguished as "white ore." It has for many years been quarried and used in the furnace along with the limonite. This "white ore" is so close-grained as to be almost flint-like in aspect, and it evidently owes its preservation from change to this quality, which renders it impervious to water. Wherever there are cracks water and air have entered, and the cracks are widely bordered with limonite. Some pieces have a thin crust of limonite—a quarter to half an inch thick—showing some progress of the alteration even over the harder portions. There are botryoidal prominences of the iron-carbonate in cavities, which seem to be protected from the change by their smooth surface as well as firm texture. This mass of "white ore" is conformable in stratification with the limestone of a hill close by the north edge of the ore-pit; and hence it was probably part of a stratum that has disappeared, over the interval between them, by alteration. I have been

informed by Mr. R. Van Buskirk, who is in charge of the mine, that this "white ore" has been taken also from the west side of the pit. There is also an outcrop of limestone in the ore-pit in which some of the layers are changed to limonite, while others above or below are unyielding—the presence of iron in any layers of the limestone (as an iron-calcium carbonate) determining their destruction.

At the Cone ore-pit, one and a half miles north of West Stockbridge, mining is done mostly through shafts; only at the north end is open mining going on so that the structure of the walls can be studied. At this end of the pit, limestone is in view having an eastward dip of 40° to 50°; and while some of the layers are not much altered, others are wholly changed to limonite. At my last visit (in 1875) the work was going on in this stratified material, the limonite portion being selected out for further separation by washing. Several layers had become wholly replaced by pure limonite, and one of those so changed was a yard thick. Some surfaces of the limestone were intersected by cracks, making areas three to six inches across, as represented in fig. 18; each crack having a border of limonite either side, an inch or so wide.

18.



The schist near by—hydromica slate—was in part firm, and in other places turned to soft grayish or yellow clay—an impure kaolin—which was silvery in surface from traces of the original mica of the slate. The quartz veins in the hydromica slate remain of course unchanged, in the ores.

The Cheever ore-bed, in Richmond, has also afforded some massive carbonate of iron, but less abundantly than the Leet ore-bed; and here, as already stated (p. 135), there is a limestone ledge near the pit.

Professor E. Hitchcock mentions the occurrence of massive carbonate of iron in Richmond, in his Report on the Geology of Massachusetts (1841), page 190, and gives an analysis showing that it contains 87·19 per cent of carbonate of iron, 5·21 carbonate of magnesium, 2·46 carbonate of manganese, 1·41 carbonate of calcium, and 2·81 of silica, alumina, etc. =99·08. The particular ore-bed in Richmond affording it is not stated, but it was probably the Leet ore-bed. In the Vermont Geological Report (1868), p. 236, Prof. E. Hitchcock gives an analysis of a dolomite from the vicinity of the Brandon ore-bed which afforded 3·61 per cent of carbonate of iron; and states that east of the bed another limestone was quarried for a flux which contained "10 per cent of iron" [carbonate?].

The limestone has occasionally a little pyrite disseminated

through it; and at one place east of the village of Great Barrington, the amount is large. But no one has reported its occurrence in the limestone about the ore-beds, and I have not myself observed it there. There is certainly not much of it.

The hydromica slate of the region contains several iron-bearing minerals: among them, pyrite (FeS_2) sparingly; pyrrhotite (Fe_7S_8) more sparingly; chlorite, often abundant, both to the north and south; garnet, often abundant in Berkshire and farther south; black mica, to the south; staurolite, rather common in Connecticut, in Salisbury and Sharon. In the decomposition of the schist all the minerals present must have contributed to the iron, for the clays contain none of them. It is probable that the schist where in near contact with the limestone also contained the iron-carbonate; but no positive evidence on this point has been obtained.

The beds all show that their formation was not dependent on erosion or transportation; and that the change to limonite required only the free access of air and moisture (with the traces of carbonic acid they ordinarily contain) to the iron-bearing materials. From some of the mines it appears that the best positions for the making of deep beds was off the mouths of valleys, or near slopes that carried down the rains for underground use. An iron-bearing carbonate would suffer rapid destruction under these conditions.

The facts which have been brought forward show that the ore-beds of Berkshire are essentially the same in nature, distribution and position with those of Vermont. The quality of the ore, the occurrence of the beds at intervals, greater or less, along the course of the limestone area, their stratigraphical relations to the limestone and schist, their origin in the oxidation of iron-bearing minerals present in the rock formations, and the concurrent decomposition of the enclosing rock or rocks—these conditions are alike for the whole area, from its northern end in Monkton, where is the most northern of the associated ore-beds, to the south extremity, near Pawling, N. Y., which place also has its large ore-bed. The facts sustain our proposition as to the geological unity of the formations. The fact that the belt of maximum ore-beds crosses obliquely the limestone area,—being near its eastern border to the north in Vermont, and near its western, west of the Taconic range, to the south,—affords additional demonstration of that unity, and suggests a close stratigraphical relation between the eastern and western sides and their formations.

The iron-bearing minerals include an iron-bearing limestone (carbonate of calcium in which iron replaces part of the calcium) both in Vermont and in Berkshire. The simple iron-

carbonate has as yet been found only in Berkshire; but its ready oxidation is a sufficient reason for its not being exposed to view even where it is or has been a prominent source of the limonite.

Most of the points in the preceding view with regard to the origin of the limonite have been recognized by different geologists; and yet the view as a whole is not so well understood that the preceding explanations are superfluous. Professor Dewey seems to have nearly reached it, as shown on page 134, and perhaps he did quite. Professor C. U. Shepard, in his Connecticut Geological Report, published in 1837, page 146, refers the limonite to the oxidation *in situ* of the pyrite and other iron-bearing minerals in the mica or hydromica slate; and he remarks that garnet and staurolites are often present in such proportions in the slates of Salisbury and Sharon as to impart to them a highly ferruginous character. Dr. Percival, his associate in the Connecticut survey, held independently the same view.

In the year 1838, Dr. R. M. S. Jackson, of Pennsylvania, as stated, with commendatory remarks, by Prof. J. P. Lesley,* announced the conclusion, after a study of the iron ores of Nittany valley and other valleys in that State, that "the brown-hematite (limonite) ore of the valleys belonged to the stratified limestone beds, and had been set free by chemical and mechanical decomposition"—making the iron to have *existed originally in the limestone as the hydrated oxide.*

Professor Hitchcock, in 1861, in the Vermont Geological Report, after stating that the limestone contained sometimes ten per cent of carbonate of iron, and referring to the occurrence of iron carbonate in the West Stockbridge ore-bed, mentioned in his Massachusetts Report, says "it is obvious that the limestones may have furnished the whole, or a part, of the iron;" but he further adds that it is probable that "the ore was derived from both the rocks [mica slate and limestone], and possibly also in some cases from the quartz rock." But to this right conclusion is added the old error, that the iron had been taken "from the subjacent rocks and re-deposited in connection with clay, sand and gravel."

The different views of other authors might be here presented if this were the place for a full discussion of the subject.

The change of the hard schists to soft clay is one of the most striking facts observed in the ore-beds. But if the iron was derived mostly from iron-bearing carbonates, a powerful agent of change was present in abundance in the carbonic acid set free by the oxidation. This acid is the most prominent source of the destruction of granite and of the reduction of its feldspar to kaolin; and in the schist it would act similarly, robbing the feldspar and mica of their alkalies, the removal

* Historical sketch of Geological explorations in Pennsylvania and other States. 200 pp. 8vo. Harrisburg, 1876, p. 81.

of which with the addition of water, makes kaolin or a more or less pure clay. Silicates, like garnet, black mica and chlorite, would yield to the same agent, and so give up their iron for oxidation. Sulphuric acid also would have been concerned in the process wherever any pyrite existed, and have aided in the destruction. But the nearly total absence from the ore of sulphur (seldom over one-tenth of a per cent*) appears to be evidence that pyrite played a very subordinate part in the production of the limonite.

It is a question of interest—Whether the iron was in the state of carbonate when the deposits were originally made by sedimentary action, or, whether in some different state, from which it was converted into carbonate as one of the results of metamorphism. The facts on page 133 under section (2) may bear on this question. Where the truth lies it is not important now to enquire. The schists were originally marine mud beds, while the limestones were clear-sea formations, an era of one condition in that region having been followed by an era of the other. Between any two such eras there appears to have been an epoch of long-continued marshes, and, while it existed, drainage depositions of iron, in some state of composition, took place over the marshes. And this feature characterized the area from north to south, though with great local differences depending on differences in depth of water and position. If the iron was deposited in those marshes as carbonate, this result was no doubt favored by the excess of carbonic acid in the Lower Silurian atmosphere and waters.

One corollary of importance seems to flow from the facts here considered. It is this: that, in some localities, beneath the richest part of a limonite bed in the direction of the dip, there *may be* iron carbonate of inexhaustible extent, and if so the regions will be good for iron ore long after the limonite beds are exhausted.

(To be continued.)

NOTE.—***Professor Dewey's determination, by chemical means, that the Taconic "talcose slate" ("magnesian slate" of Emmons) is mica slate.***—In a note to page 334 of the last volume of this Journal, the Vermont Geological Report is credited with the first announcement that the "talcose slate" of Western New England is mainly a mica slate in which the mica is a hydrous species; and on page 366 of vol. iv (1872) the analyses published in the Vermont Report are cited. It is due to the late Professor Dewey, who in early life worked in chemistry as well as in geology, mineralogy and botany, to state that in the *first* volume of this Journal, published in 1819, in an article on the geology of

* Analyses of the same ores, from similar positions, in Pennsylvania in Frazer's Geological Report, Harrisburg, 1876.

the vicinity of Williamstown, Mass. (Northwestern Berkshire), he observes that the rock of the Taconic range is mainly a "*very fine-grained mica slate*—a rock which had been called 'soapstone slate,' and appeared to be *talcose*;" "but," he adds: "I have been able to detect but a very minute quantity of magnesia in any specimens I have tried, though I obtained a considerable proportion of alumina."

In a later article (vol. viii, p. 8, 1824) he says: "The Taconic Mountain is a huge mass of mica slate." He also uses in the same article the term "talco-micaceous slate" for some portions of the rock, but still holds to the result of his chemical trial that the amount of talc was very small, and that it was strictly mica slate.

ART. XX.—*A proposed new method in Solar Spectrum Analysis*;* by S. P. LANGLEY.

No observation of modern Physical Astronomy is more striking in its conception, than that which attempts to determine the motion of a celestial body, by the altered wave-length of its light, and none has attracted more general attention. It is popularly understood, I think, that the proper motion of certain stars in the line of sight, has been thus completely demonstrated, but those particularly engaged in such studies know how far astronomers have till very lately been from the certainty attributed to them. Only last April, Father Secchi presented a communication to the Comptes Rendus† in which he recalled the denial on theoretical grounds, by Van der Willigen, of the possibility of any such observation, pointed out the extreme danger of instrumental error in such spectroscopic work, and gave experiments of his own, to show that its causes were so numerous and so subtle that it was difficult to be certain of any result.

Without discussing the replies, which aim to show that lately precautions have been taken which it may be hoped are effective, it may be observed that there is another mode of investigating the reality of the phenomena, not liable to as many difficulties as in the case of the stars, though presenting a formidable one of its own. I mean, of course, the observation of the different wave-lengths of light coming from the east and west limbs of the sun, which, owing to that body's rotation on its axis, have equatorial velocities that together make up nearly two and one-half English miles a second. This speed, enormous in itself, is most insignificant compared with the velocity of light, and this relative smallness constitutes the difficulty in

* Communicated to the meeting of the National Academy, held in Washington, April, 1877.

† C. R., tome lxxxii, pp. 761–812.

attempting the solution of the problem by means of the sun, for the whole displacement due to it, is (as Professor Young has remarked in illustration), but one-seventy-seventh of the distance between the D lines, or between one-twelfth and one-thirteenth of one division of Angström's scale. Zöllner, Secchi and Hastings have believed that they, nevertheless, detected a change in the refrangibility of the light, and Vogel* using Zöllner's reversion spectroscope obtained a displacement of from ·08 to 0·15 of one of Angström's units. In August last, Professor Young gave the results of his own measurements with one of Mr. Rutherfurd's gratings, showing an equatorial velocity of $1\cdot^{m}42$. Professor Young was unable to find any displacement of the atmospheric lines. This last research† being much more systematic than its predecessors, and given in satisfactory detail, has turned the weight of scientific opinion in favor of the view, that the change due to motion of the luminous body is fairly proven. It can hardly, however, be deemed superfluous to still offer upon so important a question, the results of an independent method of measurement, and one which renders errors from instrumental displacement, on the danger of which so much stress has been deservedly laid, in the sense in which the word is here used, not only unlikely but impossible.

In the course of a research upon the selective absorption of the solar atmosphere, I arranged in 1875, means for comparing homogeneous lights from different parts of the disc. The apparatus was too complex for description here, but it consisted essentially, in the provision of two pair of right-angled prisms of total reflection, so disposed in connection with a spectroscope, that the spectra could be formed side by side, of light from different parts of the sun, and of a photometric apparatus by which the relative intensity of the lights at different parts of these spectra could be compared. The results of this research, with an improved form of the instrument, will I hope be soon ready for publication. It was not intended, primarily, for the comparison of individual spectral lines, for which purpose optical arrangements, not very essentially different, had already been used by Lockyer, Hastings and perhaps others, but the investigation of Professor Young suggested to me another and cognate method of testing the principle of Doppler, to which this apparatus is especially applicable. The theory of the proposed method is very simple. Let two spectra be formed side by side, the one of light from one edge of the sun, the other of light from a point 180° distant. The instrument being in adjustment, if these points be in the neighborhood of the solar poles which are relatively at rest, all the lines

* Beobachtungen auf der Sternwache zu Borhkamp.

† American Journal of Science and Arts, vol. xii, Nov., 1876.

will be continuous in both spectra. But if the instrument is rotated till the light comes from points on the eastern and western sides of the sun, which are in relative motion, not only will the solar lines be discontinuous, in the two spectra; as though the one receiving light from the advancing or eastern side had been slid past its neighbor toward the violet; but any maladjustments of the instrument, which simulate this effect, can be with certainty detected by a means to be shortly described. The actual quantity by which we may anticipate from theory that the spectra are displaced, is, as has been observed, extremely small, and to produce the desired result we need not only very great dispersion in the spectroscope, but very delicate workmanship in the cutting and mounting of the prisms which are to bring the light to the slit, and in general it will be evident that more than common skill is to be desired of the instrument maker.

I wish to acknowledge the obligations I am under in these respects to Mr. F. Walther, of Philadelphia, the optician, and to Mr. William Grunow, the maker, of New York. Not to dwell on those difficulties of detail which always come between the conception and its embodiment in practice, I may say that more than six months have passed in experiment and modification till the instrument has attained its present form, in which I have been finally able within the past few days to see my anticipations justified. That it has been possible to me to undertake this research at all at present, is due to Mr. Rutherfurd, who has given me choice specimens of his gratings, which are so generally known and valued, that it is unnecessary that I should describe them.

It is desirable that a very clear mental picture should be present to the observer, of the *amount* of displacement he is to expect, for though it is well that he should be ignorant, if possible, of the anticipated direction, this knowledge of the amount will prevent him at the outset from confusing apparent displacements due to mal-adjustment of the instrument with those to which his attention should be directed.* I have taken advantage of a table prepared by Professor Pickering, showing the

* The following method of statement appears to present material for such a mental picture. In the 3d spectrum of the 17280 line grating the D lines, viewed under the magnifying power most favorable in my instrument to clear vision, appear like two sharply-defined parallel lines $\frac{1}{8}$ inch apart viewed by the naked eye at the distance of 10 inches. One of the units of Angström's scale appears to fill rather more than $\frac{1}{80}$ inch at the same distance. The displacement here due to rotation is as is remarked rather more than $\frac{1}{13}$ or ·08 of a unit, or, referred to the distance of distinct vision, very near $\frac{1}{800}$ of an inch. It is known that we can with the naked eye distinguish $\frac{1}{3000}$ of an inch or less, in the form of discontinuity of two lines on a vernier plate, which is essentially our present case. Owing to defective light and other causes, we cannot do quite this on the sun, as yet, but can still count on detecting by this method a discrepancy of somewhat less than ·03 units, which again is less than half the amount in question.

results of different instruments on the fine lines of the E group, to compare mine with them, and I observe that I can discriminate more, and more delicate lines, with either the 8,000 or 17,000 line grating than observers have been able to do with the most powerful spectroscopes of the common construction. Where Angström, using a grating of Nobert's, has delineated twelve lines, the grating supplied me by Mr. Rutherfurd shows thirty-one, and one of these pairs, though separated by but distances of one-sixth of a unit of Angström's scale, is yet clearly divided. The displacement to be expected from the solar rotation, under favorable circumstances, is about one-half of this, and hence if it exist, it is clearly within the power of the instrument to exhibit, since the two spectra are formed in juxtaposition as nice as that of a vernier plate to its circle, a circumstance under which, as every one knows, the eye judges of the continuity of lines with the most accuracy.

The instrument itself consists of the grating, with a collimating and observing telescope, each of one and one-quarter inch aperture and fourteen and one-half inches focus, with positive eye-pieces magnifying from ten to thirty times. The slit is constructed so that its jaws are bevelled to knife-edges, where they meet, at the outer surface, and these owing to the excellent workmanship, still close so as to admit no light when held up against the sun. Over this slit, with their bases almost in contact with it, and their united edges crossing it at right angles, are the first pair of reflecting prisms. They are cut from the same piece of glass, and so fitted that their junction-line shows no more in the spectrum than a particle of dust, and in fact, the division between the two spectra is with difficulty distinguishable from the ordinary dust lines. The other two prisms slide independently on ways, with verniers reading to thousandths of an inch, permitting them to be put in any part of a solar image of from one and one-half inch, to five inches in diameter. Over them is a hood, carrying a screen, which receives the image projected by the equatorial telescope. This focal image is nearly one and three-quarters inches in diameter, and any two selected portions of it pass through adjustable apertures in the screen. By the construction of the instrument they are equally distant from the optical axis, and in every other optical condition, as far as possible, similar, except as they may differ at their origin in the sun itself. It shows the difficulties of the method of research successfully employed by Mr. Huggins, and at Greenwich, that when the instrument is turned directly on the sun (i. e. so that each spectrum receives light from all parts of the sun's disc), the two sets of spectral lines will not ordinarily be continuous. Theoretically they should be, practically, we find they are not, owing to numerous latent causes

of disturbance. A touch on the prisms, a movement of the slit, an adjustment of the eye-piece, will ordinarily disturb one spectrum relatively to the other, by a minute amount. But the whole change we are seeking is of a minuter order still; how then, can we discriminate it with certainty? In our ability to do this lies the advantage of the method I describe, which, granting sufficient dispersive power, makes impossible the instrumental error that has been dwelt on by Secchi and others with justice, as shaking confidence in the result. To see how we are authorized to use this word "impossible," let us bear in mind that the solar spectrum consists of two distinct kinds of lines, one caused by absorption in the solar, the other by absorption in the terrestrial atmosphere. These latter being formed by light from all parts of the sun are independent of its rotation.

The prisms are adjusted, till, on looking on the sun directly, the lines are all continuous in both spectra, then the instrument is put in the telescope and the slit placed at such a position-angle that the light in spectrum A comes from the vicinity of the north solar pole, that in spectrum B from the south. On looking in, we see a very long and narrow spectrum, filled with dark lines and exhibiting the chromospheric lines on both sides. It is divided by what appears to be a fine dust line, in two exactly corresponding parts, and is in reality two distinct spectra, as we see by the opposed chromosphere lines, but as the sources of light for both spectra are relatively at rest, all the dark lines are still continuous. But now (without disturbing any adjustment), revolve the whole 90° about the optical axis passing through the center of the solar image, so that spectrum A is formed by light from the eastern or advancing edge of the sun; spectrum B by light from the western or retreating one. A curious change has taken place. By a very minute but perceptible quantity, spectrum A appears to have been slid past its neighbor, toward the violet end, so that every solar line in the first is "notched" at its junction with the second, while at the same time, the telluric lines are as unaltered as the fixed lines of a micrometer web would be, by moving a scale about in the field. The effect is the same as though the spectra were tangible things, like two engine-divided scales, whose numerous delicate divisions (represented by the solar lines), were all in exact juxtaposition a moment before, and are all now just perceptibly displaced, as when a vernier plate is moved till a coincidence is made at a new stroke on the limb.

Moving the instrument 90° more, we come again into the axial line of the sun, and the coincidence should return: with still 90° more we are again in the equator, but now spectrum A is formed by light from the western edge, and this time it is

moved the other way, as if it were a scale which had been slid by a very slight but distinctly perceptible amount toward the *red* end; while still the telluric lines retain their continuity, assuring us that no mal-adjustment has occurred.

It will be admitted that this change is, if real, excellent experimental evidence that the wave length is virtually different in light from the eastern and western limbs, as theory predicts. For, granting that the instrument is mal-adjusted in any unknown way or degree, any *instrumental* cause will affect solar and telluric lines alike, and we may in fact defy ingenuity to suggest an error of adjustment, which will modify one and not the other.

For the sake of clearness, I have assumed that we start with all the lines continuous in both spectra; in practice this condition is not easily assured: commonly some lurking error, will, without especial pains, cause them to appear broken upon a fixed source of light; but we disregard this, and consider, as we bring the instrument into new positions, only the *difference* of displacement of the solar and telluric lines. The simultaneous observations of this difference, in each of two spectra, is the essential condition relied on, not only in theory but in actual work.

It will be remembered that many lines in the spectrum are only seen when the sun is low. These are clearly due to absorption in our atmosphere. Many thousands, as we know, are due to absorption in the sun's atmosphere. There remains a large number of lines not coincident with any we produce at the electrodes of our battery, and always present in the spectrum. Of these we do know that they are either caused by the sun's atmosphere or ours, without always knowing which, for these can only be inferred to be telluric from their growing stronger as the sun sets,* and this, though easily determined in the case of a single line, becomes a task of great labor where we deal with thousands. It is evident, however, that after having used known telluric lines to determine the fact that the refrangibility of solar lines only is altered, we can reverse the process, and classify unhesitatingly hereafter, all lines as telluric which are unaffected by the changes that compel others to betray their solar origin. To merely see these two spectra with clearness, then, is to be enabled to pick out the telluric lines from the others, as though they were mapped before us. They are mapped in fact, and it becomes, under the proper conditions, a matter of simple inspection to determine them.

I hope then, it will not seem too assuming a title, if I speak of this as a new method in solar spectral analysis.

I have only to add, that in all my trials of this method, I have

* Or by experiment on artificial lights viewed through intervening atmospheres.

constantly so arranged each experiment, that I remained in intentional ignorance as to which spectrum came from the eastern, and which from the western limb, until I had determined the point by the different behavior of these solar and terrestrial lines, which I have been able to do thus far correctly in every instance. I believe, in fact, that the effect under proper conditions is so marked that the observer hereafter need not take pains to guard against the unconscious bias of his measurement toward a desired result. I have, in these preliminary experiments, however, thought it necessary to take precautions that no such prepossession, if it existed, might influence me unawares.

I ought to add that the proposed method is considerably limited, not only by the need of very powerful instruments (for these can be supplied), but by the need of very good observing weather, which unfortunately we have not at command when we want it.

Useful tests of the desired condition are the duplication of such lines as 1529 of Kirchoff's scale, and the more celebrated "1474," known as a close double since Professor Young's duplication of it last year.

I have not yet made any measurements of the displacement by this method, which appears to me as thus far described to be less adapted to quantitative results than that already employed by Professor Young, but capable by the use of the micrometer of giving exact numerical data. I ought to add, however, that Professor Rood, of Columbia College, has offered the valuable suggestion, that by the use of a double refracting prism, we can cause the spectra to overlap, thus not only overcoming some mechanical difficulties which I have not enlarged on, but obtaining at little trouble all the advantage of micrometric measurement. I regret that the instrument reached me so late that I have not yet been able to give a trial to this, from which I hope excellent results. Nor have I been able to introduce photography with other contemplated improvements, such as the simultaneous use of both right and left spectra of the same order, thus doubling the displacement; and the use of three reflections instead of two, on each ray before it reaches the slit, thus inverting the portion of each limb under examination, and bringing the very edges of the sun into juxtaposition, instead of a portion somewhat within the edge, as at present.

I have only to add in conclusion, that while the instruments used have been principally those of the Allegheny Observatory, to which institution my services belong, I should have been unable to carry out this research without certain indispensable adjuncts, the expense of which has been defrayed from an appropriation placed at my disposal by the trustees of the Bache fund.

Allegheny Observatory, April 14, 1877.

ART. XXI.—*Note on the Exactitude of the French Normal Fork; a Reply to the paper of Mr. A. J. Ellis;* by RUDOLPH KOENIG, Ph.D.—[In a letter to the Editors.]

AN attack, as strange as it was unexpected, has just been made in England upon the exactitude of the official French fork. Mr. Alexander J. Ellis, having found that the notes of a tonometer, composed of sixty-five harmonium reeds, constructed by Mr. Appunn, do not agree with this fork, has considered himself entitled to assert, in a paper published in the Journal of the Society of Arts (May 25, 1877) and in Nature (May 31, 1877) that the normal French fork La_3 gives, not 870 single vibrations, as has been hitherto supposed, but 878 single vibrations. Mr. Ellis, having established the further fact that the forks constructed by me are in perfect accord with the French La_3, does not hesitate to affirm that all these forks, including those even of my large tonometer, which he has probably never seen, and in any event has never examined, are necessarily inexact. Not having at my disposal the instrument used by Mr. Ellis, I confess that I find myself under some embarrassment in stating at once by what error of construction this instrument, in the hands of Mr. Ellis, has given results so extraordinary. Fortunately, I can refer to a letter from M. Helmholtz addressed to Mr. Appunn and published by the latter himself in a paper on the acoustic theories of M. Helmholtz. This letter speaks of an instrument of exactly the same character, and made by the same maker, and sufficiently explains the surprising discoveries of Mr. Ellis. "I have examined your tonometer several times," writes M. Helmholtz to Mr. Appunn, "and I am astonished at the constancy of its indications. I would not have believed that reeds could give sounds so constant as those given by your apparatus, thanks to your method of regulating the current of air. The instrument, it is true, varies a little with the temperature, as do also forks; and hence it can be used for determining the absolute number of vibrations, only when one can work in a room heated by a stove. By the aid of an astronomical chronometer, I have counted the beats, and believe that your seconds pendulum must have been slightly inexact, because, though the number of beats agree very well among themselves, the absolute number obtained is not 240 but 237 to the minute. The temperature, which was rather low during my experiments, may count for something; but even this influence may be eliminated by counting the beats to the end of a major third, which took me a quarter of an hour. In this way I have found for my Paris fork 435·01 vibrations, which agrees to the $\frac{1}{40000}$ nearly with the official number, 435 vibrations."

This letter proves that the entire number of beats in the octave of the tonometer tested by M. Helmholtz was $\frac{237}{60} \times 64 = 252{\cdot}8$, and that its fundamental note was 505·6 single vibrations instead of 512. On comparing this note of 505·6 single vibrations with a

fork giving actually 512 single vibrations, Mr. Ellis would find the latter to be 6·4 single vibrations more acute and, without doubt, would consider it as giving 518·4 single vibrations. Now for my forks giving 512 single vibrations he has found 516·7 only, with the tonometer which he used. Whence it would seem that the fundamental note of this latter instrument had become more nearly exact than that of the tonometer examined by M. Helmholtz, since the number of its vibrations is 507·3. This note, however, still remains quite distant from its true value.

The fact that M. Helmholtz succeeded, with an instrument of this sort (and one too, even less perfect than that used by Mr. Ellis) in finding the number of vibrations of the official French fork to be exact, by first determining the correction needed for his instrument, is evidence that Mr. Ellis has neglected to determine a similar correction required for his tonometer. He has too hastily declared that these small tonometers with harmonium reeds are the most perfect and the most exact in existence. It would certainly have been much better if he had first given himself a little practice in the manipulation of acoustic instruments, before having treated so slightingly the results obtained by Lissajous, by Despretz, by Helmholtz, by Mayer, etc., etc., and before seeking to throw discredit upon the labors of a *constructeur* who had no reason to expect so unjustifiable an attack.

26 Rue de Pontoise, Paris, June 5th, 1877.

[NOTE.—In response to our request, Professor A. M. Mayer has furnished us with the following statement concerning the absolute number of vibrations of Koenig's forks. "During the months of March, April and May of 1876, I made many determinations of the number of vibrations of Koenig's UT_3 fork and found that it gave 255·96 complete vibrations in one second at a temperature of 60° Fahr. The following are the separate determinations of that series of which the above number is the mean: (1) 255·95, (2) 255·97, (3) 255·90, (4) 255·92, (5) 256·02, (6) 256·02. The fork's vibratory period is accelerated or diminished $\frac{1}{22000}$ part by a difference of temperature of $\pm$ 1° Fahr."—EDS.]

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

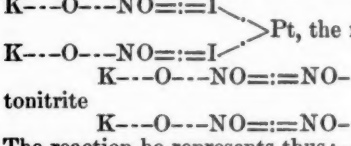
1. *On the Heat of Combustion of Oxygen and Hydrogen in closed vessels.*—THAN has modified the ice calorimeter of Bunsen so as to render it available for determining the heat of chemical action. As a first result with the new instrument—the exploding tube of which he calls a heat-eudiometer—he has obtained accurate determinations of the heat of combustion, in closed vessels, of electrolytic gas. Since, hitherto, according to him, this term has not been defined with sufficient exactness, Than uses "heat of

combustion," or more generally "total difference of energy" of a mixture of oxygen and hydrogen, as expressing that quantity of actual energy which is evolved when the gas at 0° and 760 mm. is completely converted, in a closed vessel into liquid water at 0°. As a mean of five experiments, the heat of combustion found was 2·02930 gram-calories, with a probable error of 0·0018. That is, one cubic centimeter of electrolytic gas at 0° and 760 mm. on burning to liquid water in a closed vessel, evolves 2·0293 heat units. Or, taking the atom of hydrogen as unity, a gram of hydrogen at 0° and 760 mm. uniting with the necessary quantity of oxygen in a closed vessel, and forming liquid water at 0°, produces 33·982 units of heat (gram-calories); a close agreement with the number obtained by Andrews, 33·970.—*Ber. Berl. Chem. Ges.*, x, 947, May, 1877. G. F. B.

2. *On Vapor volumes and Avogadro's Law.*—The law of Avogadro asserts that equal volumes of all substances in the gaseous state, contain the same number of molecules; from which it follows that the molecules of all bodies in the gaseous state are of the same size. Hence calling the volume of the hydrogen molecule 2, the volume of all other molecules will also be 2. While the great majority of volatile bodies conform to this law, there are some whose molecular volumes are apparently represented by 4, 6, or 8. Such bodies are phosphoric chloride, ammonium chloride, ammonium sulphydrate, chloral hydrate, etc. While those chemists who deny the universality of Avogadro's law, maintain that these bodies have actually the molecular volumes given above, those who assert this universality claim that the true molecular volume of these substances is two, as it should be, but that dissociation takes place at the temperature of the experiments, yielding two or more bodies, the molecule of each of which has a volume of two. Troost has submitted this point, in the case of chloral hydrate, to the test of experiment, in a very ingenious way, by introducing into the vapor of this body, a salt containing water, whose dissociation-tension is, as nearly as possible, equal to that of the aqueous vapor of the chloral hydrate. If the vapor of the chloral hydrate suffers dissociation and consists of equal volumes of chloral vapor and of aqueous vapor, then the salt introduced will give up no water; i. e., the vapor-volume will remain constant. But if chloral hydrate is volatile as such, its vapor is free from water and on introducing the salt it will give up water and the volume will increase till the dissociation-tension is reached. The salt chosen was potassium oxalate, $K_2C_2O_4+H_2O$, whose dissociation-tension at 78° is 53 mm. and at 100° 182 mm. The experiments were conducted in Hofmann's vapor density apparatus, in both alcohol vapor and steam. In both cases the result was the same; the volume increased on introduction of the oxalate. Hence Troost asserts that chloral hydrate is volatile without decomposition.—*Ber. Berl. Chem. Ges.*, x, 899, May, 1877. G. F. B.

3. *On Plato-diiodo-dinitrosyl and Triplato-octonitrosylic acid.*—By the action of an alcoholic solution of iodine upon potassium

or barium platonitrite, NILSON has obtained a new class of bodies which he calls platoiodnitrites. The mixture, which is at first dark reddish brown, becomes, on heating to 30° to 40°, clear amber-yellow, evolving abundance of gas, in which the characteristic odor of aldehyde is perceivable, and depositing on cooling the platoiodnitrite in crystals. The potassium salt forms large brilliant four-sided amber-yellow prisms, having the formula $K_2N_2O_4I_2Pt.(H_2O)_2$. Nilson assigns to it the rational formula



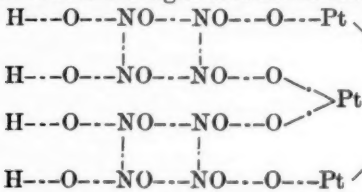
platonitrite being very simple.

The reaction he represents thus:—

$$K_2(NO_2)_4Pt+I_2+(C_2H_6O)_3=K_2N_2O_4I_2Pt+C_2H_4O+(C_2H_5ONO)_2+(H_2O)_2.$$

The radical he names plato-diiodo-dinitrosyl.

Nilson has described also a new platonitrosylic acid, obtained while endeavoring to produce platotetranitrosylic acid by the method of Lang. For this purpose barium platonitrite was decomposed by sulphuric acid in the cold, and the solution was evaporated in a vacuum over sulphuric acid. At first red needles were deposited, corresponding to the acid of Lang; but on carrying the solution to dryness, a brownish-green brilliant residue remained, which was easily soluble in water, was strongly acid, was permanent at 100°, and which after drying over sulphuric acid, had the composition $H_4(NO_2)_8Pt_3O.(H_2O)_2$. Hence the unstable tetra acid, evolving one-third its nitrogen as nitrous acid, forms a remarkably permanent octo acid, the potassium salt of which is well crystallized. The author calls it triplato-octonitrosylic acid and assigns to it the rational formula:—



—*Ber. Berl. Chem. Ges.*, x, 930, 934, May, 1877. G. F. B.

4. *On the Action of Bromine upon Pyrotartaric Acid.*—BOURGOIN has studied the action of bromine upon pyrotartaric acid, prepared as usual by the distillation of tartaric acid. For this purpose 6·5 grams were heated with 10 c.c. of bromine and 50 c.c. of water in a sealed tube to 130° or 132° for thirty-six hours, or to 152° for fifteen hours. On opening the tubes carbon dioxide was abundantly evolved, and the tube contained a dense liquid upon which rested an aqueous solution of hydrobromic acid. The heavy liquid, after washing with dilute potassium hydrate solution, was colorless and transparent, having an ethereal camphor-like odor,

irritating to the eyes, a sharp very sweet taste, a density of 2·93, and boiling about 200°, undergoing partial decomposition. On analysis it gave the formula $C_2H_2Br_4$. As it readily solidifies at −17°, it is thereby distinguished from acetylene tetrabromide, which remains liquid at −20°. From its reactions, the author regards it as tribrom-ethylene hydrobromate (or its equivalent dibrom-ethylene dibromide,) which solidifies in a freezing mixture and resembles it closely in other particulars. The third body known which has the formula $C_2H_2Br_4$, is tetrabrom-ethylene hydride, a crystalline body melting at 54·5°. The use of bromine in distinguishing the isomeric forms of pyrotartaric acid, is evident. —*Bull. Soc. Ch.*, II, xxvii, 395, May, 1877. G. F. B.

5. *On the Polybasic acids obtained by the action of Carbon dioxide on Phenol.*—The interesting synthesis of orthoxybenzoic (salicylic) and paraoxybenzoic acids by the action of carbon dioxide upon sodium-phenol and potassium-phenol, discovered by Kolbe, and the further discovery by Ost, that a second and a third carboxyl group could be in this way introduced into phenol, forming respectively a phenol-dicarbonic and a phenol-tricarbonic acid, has led the latter chemist to study carefully the conditions of the reactions. For the tricarbonic acid, the process is conducted as for salicylic acid, the mixture being finally more strongly heated —to about 360°—until phenol ceases to come over. The purification is accomplished by converting the acid into barium salt, and then decomposing this by a large excess of hot hydrochloric acid; the acid separates in crystals on cooling. From 650 grams phenol, converted into sodium-phenol and heated to 270° for thirty-two hours (when 330 grams of phenol came over) and then for fourteen hours more with a gradually increasing temperature (during which 110 grams more of phenol distilled over), 20 grams phenol-tricarbonic acid were obtained. The dicarbonic acid is best obtained by using a mixture of three molecules of sodium-phenol and one of potassium-phenol, and heating to 320°. By reason of its insolubility in water, the acid is easily purified. From 50 grams phenol, 10 to 12 grams of pure phenol-dicarbonic acid were obtained, and 32 grams of phenol were recovered. Its formation from salicylic (orthoxybenzoic) acid and its conversion into paraoxybenzoic acid on heating, prove that the two carboxyls occupy the ortho and the para positions. As to the tricarbonic acid, Ost converted it into the chlor-acid by the action of phosphoric chloride, and then reduced this by sodium amalgam; thus obtaining trimesinic acid $C_6H_3\begin{cases}COOH\\COOH\\COOH\end{cases}$ identical with that obtained from mesitylene by Fittig. The phenol-tricarbonic acid is therefore oxytrimesinic acid. No further introduction of carboxyl into phenol was possible, the meta derivatives not being obtainable in this way; a fact true also of the nitro-derivatives.—*J. pr. Ch.*, II, xv, 301, April, 1877. G. F. B.

6. *On the Relation of Cystin to Sulphates in the Urine.*—Niemann has examined a case of cystinurea and has sought to

determine the quantitative relation between cystin and other constituents of the urine, in this disease. The most noteworthy fact observed was that the per cent of cystin stood to that of the sulphuric acid as 1 : 3·89, and that the two varied together. The quantity of uric acid was diminished, but that of the urea was not sensibly affected. The patient excreted from 0·4234 to 0·595 gram cystin per day.—*Liebig's Ann.*, clxxxvii, 101, May, 1877.

G. F. B.

7. *On Gauss's Theory of Capillarity.*—The virtual work of the molecular forces engaged in the phenomenon of Capillarity is divided, according to Gauss, into: 1, that which arises from the force of gravity; 2, that which arises from the forces between the particles of the fluid; and 3, that which arises from the forces between the fluid particles and the surrounding solid bodies. The contact line between different bodies is made apparent when the complicated integrals are reduced to simpler forms. Lippman separates a quantity from the general expression. This quantity is connected with the variation in the contact line. Lippman believes that he has made this included connection "for the first time" noticeable. He discusses the changed equations of Gauss which he has thus divided into two parts. By a consideration of the facts that the volume variation of solids and in compressible fluid bodies = 0, and that by the shifting of absolute movable particles of a homogeneous fluid without volume variation, no work is done—the original equations can be simplified. Lippman finally shows why we cannot assume that the rise of a liquid in a capillary column depends upon the attraction between the fluid and the unmoistened parts of the walls of the containing vessel.—*Beiblätter, Pogg. Ann.*, vol. i, No. 5, p. 275. J. T.

8. *Influence of Light upon the Electrical resistance of Metals.*—Dr. R. Börnstein, Assistant in the Physical Institute of Heidelberg University, claims that: "The property of experiencing a diminished electrical resistance under the influence of luminous rays is not confined to the metalloids selenium and tellurium, but belongs also to platinum, gold and silver, and in all probability to metals in general. The electrical current diminishes both the conductivity and also the sensitiveness to light of its conductor; and both of these, after cessation of the current, gradually acquire their former value.—*Phil. Mag. Supplement*, June, 1877, p. 1.

J. T.

9. *Chemical and Physical Researches;* by Thomas Graham. Collected and printed for presentation only. Preface and Analytical Contents, by Dr. R. Angus Smith. Edinburgh, 1876.—What Horace wrote of his own works

"Exegi monumentum æve perennius,"

is equally true of the product of all literary and scientific labor of the highest class, and no more worthy tribute can be paid to intellectual greatness than by carefully cherishing that monument, which the noble intellect during this earthly life erects to itself.

Such a recognition, though long delayed, the French Government bestowed both on La Place and on Lavoisier, by publishing magnificent editions of their works. A similar debt of gratitude the American Academy of Arts and Sciences has paid to Rumford; but we do not hesitate to say that the elegant volume before us, in which his two friends,—James Young and R. Angus Smith,—have embodied the "Chemical and Physical Researches of Thomas Graham," is the noblest tribute of all. To the scientific contemporaries of Graham, among whom it has been widely distributed, this volume is the most worthy memorial possible of the successful student of nature whom all honored, and of the man whom all who approached him loved. His modesty of character was in striking contrast with the self-assertion of the Roman poet who could write the famous verse we have quoted above; but no friend of Graham can doubt that this literary tribute would be far more grateful to him than the bronze statue which adorns his native city. Bronze can add no luster to intellectual greatness, and however much it may swell the false glory of the living, to the eyes of posterity it only renders the transient splendor the more vain. Such monuments have their value in so far as by keeping constantly before the people examples of true greatness they serve to educate a nation. But who does not know that the chief motive which erects statues of great men is national vanity, and when will nations learn that they can serve even this selfish aim far more effectually by promoting with pecuniary means the labors of their great scholars while living, and carefully cherishing their works when dead. Every student of science would have preferred that the memorial before us, worthy as it is of one of the noblest scientific scholars which England has produced, should have been a tribute of the nation rather than an offering of two devoted friends, and all must regret that a similar tribute has not been paid to Graham's great contemporary, Michael Faraday.

The great value of memorial volumes like the one which Graham's two friends have presented *gratuitously* to the scientific world is to be found in this,—that they add finish to the completed work of the great man, they commemorate, and exhibit its unity. The investigations of almost every successful student of nature are directed by a few controlling ideas, and although the parts may seem to his contemporaries to be disconnected, because the results are published at intervals, yet when the work is done, and is seen with the perspective which time gives, the plan appears; and no greater service can be done for the departed scholar than to bring together his scattered papers, and make evident to the world the great thoughts which give unity and dignity to the whole; and not only is the work of his life thus made to appear more worthy, but, what is more important, it is rendered more useful to science.

The trait we have pointed out is most conspicuous in the investigations of Graham, and the service we have commended has been most ably rendered to his deceased friend by Dr. Angus

Smith, in the admirable "Preface and Analytical Contents" of the volume we are noticing. It is this feature which in our estimate renders this volume a more worthy memorial than either of the works to which we have referred above. In a biographical notice prepared for the Proceedings of the American Academy of Arts and Sciences, and reprinted in this Journal, III, vol. i, p. 115, we have already expressed our appreciation of the remarkably unique character of Graham's work, and shown that it forms a large part of the experimental basis of the recent science of molecular mechanics; and we are glad to find that our estimate of these investigations has been so fully confirmed. There is but one observation which we desire to add.

Faraday and Graham were two of the most successful discoverers who in any age have illustrated the annals of physical science, and their success was wholly due to the power with which they wielded the inductive method of experimental research. Experiment guided by analogy ever has been, and ever must be, the fundamental condition of all progress in physics, and we regret to see a tendency to undervalue this mode of investigation as compared with the more rigid methods of mathematical deduction. The ability to devise crucial experiments, and to overcome the difficulties which the material conditions of every new physical problem present, is a talent of fully as high an order as that which can marshal differential equations and draw forth the truths which they involve; and this mathematical skill, all important as it is, and essential to the later developments of the science, would be powerless without the data, which experiment has supplied. We would therefore most earnestly commend to the attention of the students of physics, these experimental researches of Graham, as an example of what great results can be attained by purely and very simple experimental means. J. P. C., JR.

Newport, July 9th.

II. Geology and Mineralogy.

1. *Bulletin, Vol. iii, No. 3, of Hayden's Expedition.*—The publication of this Bulletin was announced in the preceding number of this Journal. The following are notes from the volume.

Professor Cope refers the Lignitic beds of the Judith River region to the Cretaceous, making the Eocene Tertiary commence with the Wahsatch group. These Judith River beds overlie the Fox Hills group, which is the Upper Cretaceous No. 5, of Hayden & Meek, a rusty sandstone separating the former (which he makes Cretaceous No. 6), from the black shales of the latter. These Judith beds have a thickness of 300 to 500 feet, and afford, above as well as below, various Dinosaurian remains. From the upper two-thirds of a section of 332 feet, Dr. Cope enumerates the following: *Lælaps*, 6 species; *Troödon*, 1; *Aublysodon*, 2; 12 others of the genera *Zapsalis*, *Paronychodon*, *Palæoscincus*, *Dysganus*, *Diclonius*, *Trachodon*, *Monoclonius*; also species of *Crocodilus*; of the Testudinate genera *Trionyx*, *Plastomenus*, *Polythorax*, *Comp-*

semys, *Emys*; of *Rhynchocephalia*, 4 species; *Batrachia Urodela*, 5; others of *Lepidosteus*, and *Ceratodus*.

Mr. Cope makes the Lignitic formation include the Judith River and Fort Union epochs, as defined by Meek, and regards the Laramie or Bitter Creek beds as representing other epochs.

Prof. White discusses the age of the Lignitic group in connection with his review of the species of fresh-water and land shells. He refers the Judith River and Laramie groups to the *Post-Cretaceous*, and recognizes the same five Cretaceous groups laid down by Hayden and Meek; but he makes the Cenozoic or Tertiary commence near the base of the Fort Union group, in the Upper Missouri region, and also near that of the Wahsatch in the Green River region; so that the Tertiary of the Green River region includes (beginning below) the Wahsatch (except its earliest part), Green River, Bridger and Brown's Park groups, all of fresh water origin; and that of the Upper Missouri River region, the Fort Union, Wind River and White River groups. The Laramie group and the lower portion of the overlying Wahsatch (some 500 or 600 feet) is the portion of the series about which the age, whether Cretaceous or Tertiary, is in dispute. The final change from brackish to fresh-water formations takes place at the close of this early part of the Wahsatch.

Dr. Peale describes mountains consisting partly of mixed eruptive and sedimentary rocks, occurring in Colorado. Among the examples of them there are the Spanish Peaks, two isolated mountains east of the Sangre de Cristo Range; two areas of low hills in Middle Park; Mount Guyot and Silverheels, northeast of South Park; four Elk Mountain areas, the Italian, White Rock, Snow Mass and Sopris; the Sierra la Sal, 120 miles west of the Elk Mountains, and others. The areas are isolated, and in regions of sedimentary rocks. The mountains consist partly of trachyte and rhyolyte, or of granite, into which the trachyte graduates, or less commonly of dioryte. The sedimentary rocks are intersected by dikes, and the beds have sometimes been carried up, broken off, overturned, and forced apart by intrusive sheets; or the liquid rock has forced its way through the strata and spread out with scarcely any displacements; or, on reaching the shales, it has pushed its way between the layers, lifting those above. The sediments adjoining are sometimes altered and even disguised, in other cases they are unaltered by the heat. The rocks penetrated and overlaid are in part the Cretaceous; and in one of the West Elk Mountain areas, Upper Cretaceous; the eruptions were certainly post-Cretaceous. In Park View Mountain and Spanish Peaks, Lignitic strata are intersected by trachytic dikes. Dr. Peale concludes that the eruptions are all of the same age; that they occurred after the elevation of the mountain ranges; and that the trachyte and other rocks have resulted from the fusion of metamorphic rocks lying beneath—the fusion being due to the heat occasioned by the movements and plication.

A Fourth edition of the "Lists of Elevations" by H. Gannett has been issued. It contains the Hypsometric map noticed on page 387 of the last volume of this Journal.

2. *The Coal Mines of the Western Coast of the United States;* by W. A. GOODYEAR, Mining Engineer. 154 pp. 12mo. San Francisco. 1877. (A. L. Bancroft & Co.)—The author of this volume has had a ten years' intimate practical acquaintance with the coal mines of California, Oregon and Washington Territory. His work gives a clear and full description of the coal fields (none of which are older than Cretaceous), and of the mines themselves as they exist to-day, together with statistics of production, information concerning the relative value for steam purposes of the various coals which come to the San Francisco market, and other cognate matters interesting not only to geologists and mining engineers, but also to all who are connected with the coal trade. The relative value of the different coal fields is considered, and the following conclusion reached: that the mines of California cannot be relied on for much more coal; that those of Coos Bay, Oregon, the only ones yet worked in that State, will not probably be worked with a profit many years longer; that "it is unquestionably to the mines of Washington Territory and of British Columbia that the Pacific coast must look hereafter, both for its chief domestic, and its nearest and most reliable foreign, supplies of a good article of coal."

3. *On the Origin of Kames or Eskers in New Hampshire;* by WARREN UPHAM, of Nashua, N. H. (Proc. Amer. Assoc., Buffalo Meeting, 1876).—Mr. Upham describes "kames" as existing along the lower part of the valleys of almost every river in the State of New Hampshire; and as being ridges several miles in length, or remnants of them, made up of sand and fine or coarse gravel, and in some places containing occasional angular bowlders. A single continuous "kame," 150 to 250 feet high, is said to extend in the Connecticut valley from Lyme, N. H., to Windsor, Vt., a distance of twenty-four miles. Mr. Upham describes the terraces as consisting of sand, clay and fine gravel, and as covering in places the kames, and therefore newer than the kames. Their origin is referred to depositions by sub-glacial streams of the material dropped by the melting glacier before its retreat.

4. *The American Palæozoic Fossils.* A Catalogue of the Genera and Species, with names of authors, dates, places of publication, Groups of Rocks in which found, and the Etymology and Signification of the words, and an Introduction devoted to the Stratigraphical Geology of the Palæozoic rocks; by S. A. MILLER. 254 pp. 8vo. Cincinnati, 1877. Published by the Author.—This title gives the contents of Mr. Miller's work. The Catalogue is the result of a large amount of labor, and will be of great service to students in Paleontology. The Introductory chapter is judiciously drawn up; and so also a preceding chapter on the construction of scientific names in Paleontology, by Prof. E. W. Claypole, of Antioch College. The Catalogue commences with the names of fossil Plants; and continues with those of Animals, beginning with the lowest. Among Vertebrates it includes only the fossil Fishes and Reptiles. A valuable addition to such a catalogue would be a table containing the titles of the works referred to.

5. *Heights of Mountains in Western Connecticut.*—The following is a list of some of the mountain elevations of Western Connecticut, as obtained by a survey made two or three years ago by Messrs. Fyler and Civil Engineer George M. Bradford, of Winsted, and the Hon. Robbins Battell and H. P. Lawrence, of Norfolk. It will be seen that Salisbury, the northwest corner township, has elevations much higher than that of Mt. Ivy in Goshen, which used to be called the highest land in Connecticut. The elevations here given are the heights above tide water:

Spaulding's Summit, Norfolk	1,336 feet.
Platt Mountain, Winchester	1,460 "
Chamberlain Mountain, Winchester	1,480 "
Ivy Mountain, Goshen	1,642 "
Riggs Mountain, Norfolk	1,565 "
Knapp Mountain, Norfolk	1,617 "
Moses Mountain, Norfolk	1,645 "
Dutton Mountain, Norfolk	1,672 "
Summer Mountain, Norfolk	1,672 "
Haystack Mountain, Norfolk	1,672 "
Gaylord Mountain, Norfolk	1,717 "
Bald Mountain, Norfolk	1,770 "
Winrow Mountain, Norfolk	1,770 "
Mt. Bradford, Canaan	1,960 "
Clipper Mountain, Canaan	1,810 "
Bald Peak, Salisbury	1,996 "
Buck Mountain, Salisbury	2,150 "
Bear Mountain, Salisbury	2,250 "
Mount Brace	2,300 "

"The last three heights are estimated." The report of the survey says: Mount Brace and Bear are 150 to 200 feet higher than the ground they stood on, [Bald Peak, Salisbury], and unquestionably in the limits of this State,—while there is some dispute as to whether the highest point of Mount Brace, which is considerably above the other two, is in this State, or just over the line in New York. The monument to mark the State line is on this mountain near the top, and it is exceedingly probable that the highest land in the State is *exactly* in the corner where New York and Massachusetts join Connecticut. Only this seems now certain, that there is land in this State that will vary little, if any, from 2,300 feet above the sea level.—*Hartford Times*, June 14, 1877. This high land is in the Taconic range and just south of Mount Washington.

6. *On some of the conditions influencing the projection of discrete solid materials from Volcanoes, and on the mode in which Pompeii was overwhelmed.*—Robert Mallett, F.R.S., has an important paper on this subject in the Journal of the Royal Geological Society of Ireland, vol. xiv, part 3, which was read before the Society a year since.

7. *The Geological Survey of Portugal.* — The recent publications of the Survey of Portugal include a memoir on the Caves of Casareda containing human remains, by J. I. N. Delgado; on the existence of Silurian rocks in the vicinity of Alemtejo, by the same; on the Quaternary of the hydrographic basins of Tejo and Sado, by C. Ribiero; on the supply of Lisbon with spring and river waters, by the same.

C. Ribeiro has also published a memoir on flint and quartz implements from the Tertiary and Quaternary of Tejo and Sado, in the Memoirs of the Lisbon Academy of Sciences.

8. *Earthquake of Jalisco, Mexico, and eruption of the Volcano Ceboruco.*—A detailed account of the Jalisco earthquake, of the 11th of February, 1877, shaking heavily the city of Guadalajara, and of Ceboruco, a volcano in the region, by a Government Commission, consisting of M. Iglesias, M. Bárcena, and J. I. Matute, is contained in the first volume of the Anales del Ministerio de Fomento de la Republica Mexicana, Tomo i, Febrero de 1877.

9. *West Rock at New Haven, Connecticut, not the termination of the Green Mountain range.*—The ridiculous statement,* that the Green Mountains terminate in West Rock, is still afloat in the School Geographies studied in and out of New England. The following correction of it appeared nearly sixty years since, in Amos Eaton's "Index to the Geology of the United States," (p. 103) a small volume published at Troy, New York, in 1820.

"It is very strange that several publications have located the southern extremity of this primitive range (the Green Mountains) in West Rock, New Haven, which is greenstone resting on red sandstone. Such random guesses given as facts are very injurious to the science."

III. Botany and Zoology.

1. *Some points of Botanical Nomenclature.*—The series of laws of nomenclature, revised and expounded by Alph. DeCandolle, and adopted at a Botanical Congress in France a few years ago,† naturally left certain practical points unsettled, or apparently so. A Belgian botanist, A. Cogniaux, asks some questions, all rising out of the way in which genera, their synonyms, and some species are succinctly dealt with or referred to in the Genera Plantarum now in course of publication; and he very naturally addresses these questions to M. DeCandolle. The latter replies, laying down the law: and the correspondence is printed in the *Bulletin de la Société Royale de Botanique de Belgique*, 1876, pp. 477–485.

The difficulty upon which M. Cogniaux fell was not very formidable, but it is one which a botanical writer has to settle, and which ought, if possible, to be settled with unanimity, and upon intelligible principles. DeCandolle seizes the principle and applies it to the case. The settlement should be satisfactory to all who really accept the principle. It may well satisfy us; for the principle is one which for many years we have strenuously maintained in this Journal. The fact is, that the name of an author, or its abridgment, appended to the name of a genus, or to a specific name, is mere bibliography, stands in the place of a citation of author, work, page, etc. For some purposes the whole citation is needed: for others the first word suffices. "When Dr.

* See this Journal, III, x, 498, xi, 151, 1876.

† Reprinted, with comments, in this Journal, July, 1868.

Hooker, in the Genera Plantarum, characterizes a new genus, *Cerasiocarpum*, and states that it is founded upon the *Æchmandra Zeylanica* of Thwaites, but does not write *Cerasiocarpum Zeylanicum*, how is a succeeding writer to refer to this record? Shall he write *Cerasiocarpum Zeylanicum*, Hook. f.?" If this were the only kind of case coming under the rule, we should say it were best to do so,—that, although Dr. Hooker has not imposed the specific name in question in fact, he has done it by such direct construction, that, in the absence of all reason to the contrary, the name might be cited in that manner. But constructions grow; and this one may form the foundation of a superstructure which would stand very much in the way. Suppose, asks M. Cogniaux, the new genus consisted of two or more species, already published under other generic names, and these were in like manner enumerated, some with certainty, some with doubt; suppose that several species are referred to a newly proposed genus by their old names, without saying whether they are severally regarded as distinct species or not; suppose, to take an actual case, a genus is said to contain only "*two or three*" known species, while four or five species of authors under other genera are said to belong to it; suppose, to take a common case, the species of a newly-formed or newly-limited genus could not be transferred bodily from their former associations to the new without re-adjustment and alteration of some names; and finally, take *Peucadanum*, into which Bentham thrusts twenty or thirty received genera, some of them numerous in species, what is to be done? DeCandolle rightly answers these questions, first, by calling attention to the fact that, from the time of Clusius and Dodoens down to Linnæus, this suffix of authors' names is merely the commencement of, or in lieu of, a citation; that it is not a matter of homage or sentiment, or justice, but a matter of fact, i. e., of historical record. The guiding principle as to this record is, that we are not to make an author say that which he has not said. The fact is that Hooker has established the genus *Cerasiocarpum*, and has not said that the species referred to it should be called *C. Zeylanicum*. Probably that is the name which the plant should bear: possibly not. When some writer describes or enumerates it under that name, this will also be matter of record. This rule, which rigid consistency requires to be followed in such a case, is obviously the only safe one in all the following cases. It is the rule which we ourselves have endeavored to follow. It is attended by a certain amount of inconvenience in certain cases, but any serious departure from it is sure to introduce confusion and falsification of the record. But it is every way desirable that the founder of a genus should complete the nomenclature of the species at the time, whenever and so far as he can, instead of leaving it to others.

DeCandolle closes with a reminder that the same considerations apply to the raising or lowering of the grade of a name; for instance, that Endlicher should not have written *Ordo Swartzieæ*, DC., when, in the Prodromus, the rank assigned is *Subordo;*

also that, when a genus is reduced to a section of some other, we should not write after it the name of an author who established it as a genus, etc. Consistency and exact correctness require that the rule *suum cuique* should be followed even in these matters. But we do not agree in the requisition that the name of the author who first used a former generic name as that of a section, should always be appended. Sometimes it is not easy to ascertain this, or to know whether or not it has been yet so employed. And when known, we may in many cases, in print as well as in writing, innocently and safely omit the authority of a sub-genus or section, as we may that of a genus or species. A. G.

2. *Athamantha Chinensis* L.—This is a puzzle, of which a probable solution by Muhlenberg has recently turned up. The following is the character, etc., in the Species Plantarum, ed. 1, p. 245.

"*Athamantha Chinensis*, seminibus membranaceo-striatis, foliis supra-decompositis lævibus multifidis.

"Habitat . . . Chinensem dixit Barthram qui semina misit ex Virginia.

"Caulis angulatus, lævis, erectus, parum flexuosus. Folia Chærophylli, lavia. Umbella minus expansa, alba. Semina singula 5-alis longitudinalibus parvis; involucrum duplex."

In the second edition of the Species Plantarum it is added: "Statura Selini Monnieri."

In reading the botanical correspondence of Zaccheus Collins, which (as is already noted in this Journal) is preserved in the library of the Academy of Natural Sciences, Philadelphia, I found the following in a letter from Muhlenberg, dated Oct. 12, 1813.

"Among the specimens from Genessee, sent by Mr. Whitlow, there is an umbellate agreeing with *Athamantha Chinensis* L., which Linnæus had from Mr. Bartram, who had visited the Lakes. Can we find out whether in Bartram's time the name of Genessee was known, and how Bartram spelled it: perhaps *Chinesee*, and whether such a word might have been misunderstood? I have never seen Bartram's journal."

The journal referred to must be John Bartram's "Observations made in his Travels from Pennsylvania to Canada." 8vo. London, 1751. The name of Genessee is certainly older than Bartram's time.

If Muhlenberg's suggestion is the right one, as is probable, the plant is probably *Conioselinum Canadense*. Yet that hardly posseses an "involucrum duplex." There is, perhaps, a specimen in the Linnæan herbarium, which would settle this point. A. G.

3. *Repertorium Annum Literaturæ Botanicæ periodicæ curarunt* G. C. W. Brohnensieg, Custos biblioth. Soc. Teylerianæ, et W. Burck, Math. Mag. et Phil. Nat. Doct. Harlemi, Erven Loosjes, 8vo.—We have before us tomm. II, 1876, and tomm. III, 1877, relating to publications of the years 1873 and 1874 respectively. Vol. I, it appears was brought out by Van Bemmelen, who sickened and died soon after its appearance. The present authors taking up the work, appear to have done it very conscientiously

and well. They put on record, in a systematic way, the titles of all the communications relating to Botany which are found in the periodicals and serials of the years in question. The contents are arranged under the heads of General Morphology, Special Morphology of the several classes and orders, Physiology, Plant-descriptions, Floras, Geography of Plants, Paleontology, etc., etc. A list of the periodicals used is given, an alphabetical index of authors, and another of Families and Genera. The work appears to be under the patronage of the Teylerian Society of Harlem. It makes a valuable supplement or continuation, so far as concerns botany, of the Royal Society's Catalogue, and has also the advantage of systematic arrangement. A. G.

4. *Sir Joseph Dalton Hooker and party on a Botanical excursion to the Rocky Mountains and California.*—Sir Joseph Dalton Hooker, President of the Royal Society, and Director of the Royal Gardens, Kew, arrived at Boston in the Parthia, on the 9th ult. He is accompanied by Major General Strachey, of the Royal Engineers, and of the India Board, the explorer and surveyor of Kamoun, etc. They proceeded at once to the Rocky Mountains of Colorado and Wyoming, in company with Professor Gray of Cambridge, and Professor Leidy of Philadelphia, escorted by Dr. Hayden, to whose Survey the party are temporarily annexed, while they make a botanical reconnoissance of the Rocky Mountain region, which may probably be extended to California.

5. *The Influence of Physical Conditions in the Genesis of Species;* by JOEL A. ALLEN. 32 pp. 8vo. (From the Radical Review, vol. i, No. 1, May, 1877.)—Prof. Allen has brought together in this paper a large number of facts—partly of his own observation—on the differences among the individuals of species connected with their geographical distribution. He draws his illustrations from birds and mammals. The differences are differences in size of body, size and form of peripheral parts, kind and intensity of color, and, to some extent, in qualities still more fundamental:—differences that had led to the establishment of many distinct species which now are known to shade into one another by hardly perceptible gradations. The author after pointing out many examples, argues that the direct influence of climate or geographical conditions—and not natural selection—is the chief source of the variations, a conclusion the facts clearly establish.

6. *Sir Wyville Thomson, and the working up of the "Challenger" collections.*—The May number of the Annals and Magazine of Natural History contains an article by Professor P. M. Duncan charging Sir Wyville Thomson with partiality and lack of patriotism in the distribution of the "Challenger" collections for description. Mr. Duncan refers especially to the Echini, Sponges, and Protozoa, a portion of which have been sent for description to America and Germany, which, according to him, belong of right to British naturalists.

If Sir Wyville Thomson has neglected any one's interests in the matter, he may be said to have disregarded his own, by sending

part of the Sponges to Oscar Schmidt; for surely no one has a better claim than Sir Wyville himself, to whatever distinction may be gained in working them up as a whole. Mr. Duncan speaks of the destination of the Echini as a wrong done to Dr. Thomas Wright, and that of the Radiolaria as a slight upon Mr. Carter. Without discussing the merits of any English naturalist as compared with those selected, it certainly is no slight to any investigator of the Protozoa, that the Radiolaria should have been intrusted to Haeckel. And we may suppose that in sending the Echini to be worked up at Cambridge, Sir Wyville was strongly influenced by the fact that a larger amount of material for the comparison of recent Echini is to be found at the Museum of Comparative Zoology than in any other institution; which circumstance, added to Mr. Agassiz's familiarity with the subject, gives him unusual facility for the work.

More important, however, than any personal interest is the principle involved in this distribution. The same cosmopolitan spirit has guided Sir Wyville on previous occasions. A reference to past transactions shows not only that he has been consistent with himself, but that he could scarcely have acted otherwise if the best and fullest scientific results were to be obtained from the materials in question. On the return of the Porcupine in 1869, it was agreed between Sir Wyville Thomson and Mr. Agassiz, then acting for the Coast Survey under whose auspices similar deep sea dredgings had been made, that they should send their respective materials, as far as possible, to the same persons, and secure the best coöperation by choosing specialists of known ability regardless of nationality. We may add, that a few years later, when the collections of the Hassler expedition were ready for distribution, the same policy was continued with even greater disregard of national predilections than in the case of the "Challenger," the material being divided among sixteen specialists, twelve of whom were either Swedes, Frenchmen, Englishmen, or Germans.

We cannot regret that Sir Wyville Thomson in distributing the invaluable collections of the Challenger has persisted in the same course. In so doing he has selected, as far as possible, those investigators whose familiarity with the results of his own earlier deep-sea dredgings, and with those of the United States Coast Survey, gives them an unquestionable advantage in working up these later collections. In short, it seems to us, that in this final division, Sir Wyville has manifested the discriminating judgment which has made the Challenger expedition an exceptional one throughout; exceptional, not as Mr. Duncan thinks it should have been in its exclusively national character, but exceptional in well laid and admirably executed plans, conceived in a liberality of spirit which lays the whole scientific world under obligation.

7. *American Addresses, with a Lecture on the study of Biology;* by THOMAS H. HUXLEY. 164 pp. 8vo. London, 1877. (Macmillan & Co.)—These addresses include those delivered by Professor Huxley in New York, in 1876, noticed on page 399 of volume xii of this Journal (1876), and also his address on the occasion of the

opening of the Johns Hopkins University, delivered at Baltimore on the 12th of September, 1876, and a lecture on the study of Biology, in connection with the Loan Collection of Scientific Apparatus, South Kensington Museum, delivered December 16, 1876.

IV. Astronomy.

1. *Notice of the Meteor of June* 12, 1877; by Professor Daniel Kirkwood. (Communicated.)—On the evening of June 12, 1877, about fifteen minutes before 9 o'clock, a large meteor was seen in Indiana, throughout almost the entire extent of the State. The *Plymouth Democrat** of Thursday, June 14th, contains the following notice of the phenomenon:

"A remarkably brilliant meteor, of a bluish color, passed through the heavens from west to east, last Tuesday evening at about fifteen minutes past 9 o'clock. The movement of the meteor was much slower than is usual with such bodies, being visible for several seconds. It left no trace of its course, as some former meteors have done. Its elevation was about thirty degrees, and the space traversed was about twenty degrees, according to the opinion of a city paper."

The time here given, fifteen minutes *past* 9 o'clock, is certainly erroneous, as the meteor was seen *before* 9 both in Muncie and Bloomington. The altitude is probably that at which the body disappeared. According to the *Muncie Times* of June 13, the meteor as seen in Delaware County was in the north, moving from west to east, nearly parallel to the horizon, at an elevation of about 40°. Mr. F. M. Parker, a graduate of Indiana University, observed it carefully at Bloomington, Monroe County. When first seen it was about 15° east of north at an altitude of 17° or 18°; its apparent magnitude was one-fourth that of the moon; its motion eastward; the length of its visible track, about 20°; and it disappeared at an elevation of 12° or 13°. With the exception of the Bloomington observations the data are very uncertain. They justify the conclusion, however, that the meteor's motion was direct, and that its height above the earth's surface at the moment of its disappearance was more than thirty miles and less than forty.

Bloomington, Indiana, July 2.

2. *Micrometric measurement of Double Stars.*—A memoir on this subject, extending to 266 pages, by N. C. Dunér, is contained in the Acta Universitatis Lundensis, tom. xii, 1875–76.

3. We are glad to learn that Mr. Burnham has been granted again the use of the large telescope in the Chicago Observatory.

4. *Handbook of Descriptive Astronomy;* by George F. Chambers. 3d edition. Clarendon Press Series. Macmillan & Co. London: 1877. 8vo, 960 pp.—The first edition of this excellent work was published about ten years ago and is probably known to most amateur astronomers. In that edition there were

* Published at Plymouth, Marshall County, near the northern boundary of the State.

several very serious deficiencies. The present edition is much enlarged, and these deficiencies have been supplied. Some of the chapters are, from the increase of our knowledge, almost entirely new. The net increase in the volume is not less than 200 pages. The work is, as its title implies, strictly *descriptive*. The author has in general avoided all theoretical discussions. The law of universal gravitation and the nebular hypothesis are alike omitted. The same is true of spherical astronomy and all methods of computation except the very simplest. The volume is therefore a handbook of the known facts of Astronomy, stated with the least possible amount of mathematics. It is not designed or fitted to be a textbook, but it is not unsuited to general reading, and the amount of information crowded into its pages makes it extremely convenient for reference by the teacher and almost indispensable to the amateur observer.

V. Miscellaneous Scientific Intelligence.

1. *Abstract of a pamphlet entitled Reflections sur les Chronometres*, par J. A. Rouyaux, Enseigne de Vaisseau. Translated by Lieut.-Com. C. H. Davis, U. S. N.*

Part I—Contains an exposé of numerous inconveniences in the direct application of formulæ to chronometers at sea, and a resumé of the advantages to be derived from an application of the formulæ to the *differences of rates* observed daily at sea.

Part II—Contains a demonstration of some properties of lines of equal rates and temperatures (lignes isothermes et lignes isomarches).

As a necessary corollary follows an examination of the results furnished, during a cruise of thirty months, by the chronometers of the *Decrès*. This examination, which in general confirms the views of Yvon Villarceau, reveals a nearly new fact: the influence which the action of the screw propellor may have on chronometer rates. A final note contains the analytical demonstrations involved in the work.

Part I.—The object of all researches on chronometer rates is a double one. 1. By a comparison of dates and temperatures, with corresponding rates, to find a law which governs these variables. 2. This law found, to apply it in such a way as to determine the rate for a given date and temperature. The common method of proceeding is, by means of careful observations, to determine rates as often as possible, the temperatures being observed at the same time. When a sufficient number of observations has been accumulated, to form a number of equations of condition, and from them determine the values of the coëfficients in the formula chosen as best applicable to the case in point. Another method

* This abstract of one of the latest contributions to an important subject is given with a view to make public a research which would perhaps have little currency in this country, and without committing the translator to the views therein expressed, from some of which his own investigations have led him to differ.

is to record the rates observed, with corresponding temperatures, in the graphic form of curves.

The inconveniences of these two methods as applied to chronometers on board ship are:

(1.) The length of time necessarily involved in the preliminary investigation: for in order to have a sufficient number of equations of condition there must be great differences in the observed temperatures, and these differences will only appear with change of seasons.

(2.) There must frequently occur, on board a cruising ship, interruptions of the regular observations.

(3.) In consequence of these interruptions the observations may be badly distributed on the scale of temperatures.

(4.) Skill is necessary in the observer.

(5.) The observations so laboriously collected and studied represent only the *Harbor rates*, whereas the practical navigator is directly concerned with the *Sea rates*, and it is well known that these two sometimes differ widely.

The navigator on considering these many and important inconveniences will be led justly to conclude that so much labor is practically useless, and will trust as implicitly to the regularity of his chronometers, as to the demonstrations of a formula based on such uncertain conditions.

It is proposed, therefore, to apply the formula, not to the rates themselves, but to their differences, or what is the same thing to the second differences of daily comparisons. These differences, when three chronometers are used, may be obtained daily, without regard to the actual value of the rates, both at sea and in port.

The length of time may be diminished, as observations are furnished every day; the interruptions in the observations will not occur; the observations will be better distributed on the scale of temperatures; talent of observation will not be necessary; two chronometers will be followed at once; and finally the perturbations of sea rates appear directly.

There is one decided inconvenience in this method, the importance of which it is impossible to deny, viz: that the result practically useful to the navigator is the variation of *the rate itself*, which must still be sought by the ordinary methods, because this method only furnishes the variation in *difference of rate*. The ordinary methods may however be facilitated by a study of the difference of rates, and are of themselves insufficient to control the regularity of sea rates or the validity of the proposed formulæ.

Since the rate of a chronometer may be regarded as a function of the two variables, time and temperature, it may be expressed by the development of a series (Taylor's theorem), and Mr. Yvon Villarceau was the first to apply this solution to the problem. His formula is the general one for all chronometers. It is shown that the formula may be applied to express the *difference of rate* of two chronometers, exposed to the same temperature, at the same times.

Part II.—Several properties of chronometer-rates are implicitly contained in the formula of Yvon Villarceau. The importance of

these properties is that they are *necessary* and *sufficient* consequences of the formula, and are consequently verified or not verified at the same time as the formula, and are capable of replacing it either as proof of an admitted law, or as a control on the regularity of rates. Their practical utility consists in the fact that their graphic construction is much easier than that of the formula.

Without entering into their mathematical demonstration these properties may be briefly stated as follows:

(1.) All the rates corresponding to the same temperature are distributed on a parabola having its axis parallel to the axis of rates.

(2.) The points corresponding to the same value of rate are all situated on a conic, and all the conics obtained for different values of rate have the same center and are similar.

[It must be borne in mind that the points corresponding to equal temperatures are points on the curve of rates, and the points corresponding to equal rates are points on the curve of temperatures.—*Translator.*]

By giving to the curves of temperatures and rates the same axis of time these properties will appear in a graphic construction of the curves.

A study of the chronometers of the *Decrès* is given to show the influence of vibrations caused by the screw of a steam vessel on the rates of chronometers on board.

A practical way of correcting rates at sea, by the application of the formula to their differences, is as follows:

Suppose that the normal values of the rates a, b, c, . . . and their differences $b-a$, $c-a$, . . . have been determined, for a given time and temperature [by any adopted formula].

If time and temperature alone affected the rates these determinations might be adopted as the correct *sea rate.* Compare the differences thus deduced with the second differences in the daily comparisons actually observed for the corresponding date. To deduce variations in the rates themselves we may reason as follows:

(1.) If none of the differences $b-a$, $c-a$, . . . have varied it may be admitted that the rate itself has not varied. Because, if the rate has varied it is almost impossible that the perturbations da, db, dc, . . . should all be equal and of the same value.

(2.) If one or several of the differences have not varied it may be admitted that the rates on which they depend have not varied. For in order that a difference $b-a$ should remain constant, while b and a both change, it is necessary that $da=db$, which is improbable.

(3.) If all the differences have varied there is no longer any probable rule to govern the corrections. In this case the change is probably due to the action of the screw, and if the effect of this action is once determined, it may be the same in a second case.

E. S. H.

2. *Earthquake wave of May 9th and 10th.*—This wave, some account of which is given on page 77, reached New South Wales

(according to a report from the Pacific Steamer, Australia) on the morning of the 11th of May (Australian time). At $5^h\ 20^s$ A. M. the tide guage at Fort Denison recorded the first of the series of waves. The oscillations continued through the day and reached their maximum at 2 P. M. the height then being three feet six inches. Telegrams from New Zealand report that similar waves were felt on the east coast, from the Bay of Islands to the bluff, commencing at 5 o'clock A. M. The maximum height was six feet.

3. *Tides of the Arctic Seas.*—These tides are mathematically discussed by Prof. S. Haughton in papers published in the Proceedings of the Royal Society, the last of which (Part vii) was read on the 17th of February last.

4. *Massachusetts Institute of Technology, Boston.*—In connection with this excellent institution, laboratories for the instruction exclusively of women were open for occupancy on the 23d of October last, and have been in successful operation through the winter and spring. The design is to afford facilities for the advanced study of Chemical Analysis, Mineralogy and Chemistry as related to Vegetable and Animal Physiology and to the Industrial Arts. The laboratories are excellent in arrangement and are furnished with all needed apparatus. The laboratory is under the charge and instruction of Professor John M. Ordway and Mrs. Robert H. Richards. The terms are $200 a year for a full term of eight months, six days per week. Students are also taken for one or two days per week.

5. *Transfer of the Shepard Collections to Amherst College.*—The scientific resources of Amherst College have recently been greatly increased by the acquisition of the large collections of minerals, meteorites, fossils, plants, etc., of Prof. C. U. Shepard. These collections have been for more than twenty-five years deposited in the Amherst College Museum; and while there they have been ever increasing in extent through the zeal and liberal outlay of Professor Shepard, until finally they had reached a value, according to his estimate, of full seventy thousand dollars, excluding the collections in zoology, botany and archæology. From good will to the Institution, with which Professor Shepard has long been connected, the whole are now transferred to the college for little more than half this sum. The collection of meteorites ranks fourth in the world, and no institution in this country possesses a superior collection of minerals.

6. *Imperial Academy of Sciences of St. Petersburg.*—This Academy has published Part I. of a detailed index volume to its publications, covering 490 8vo pages and including the articles in foreign languages.

7. *National Academy of Sciences.*—The Academy has recently published vol. I, of Biographical Memoirs, 344 pp. 8vo, containing memoirs of J. S. Hubbard, J. G. Totten, B. Silliman, E. Hitchcock, J. M. Gilliss, A. D. Bache, J. H. Alexander, W. Chauvenet, J. F. Frazer, J. H. Coffin, J. Torrey, W. S. Sullivant, J. Saxton, H. J. Clark and J. Winlock. Also volume I, of Proceedings of the Academy. 120 pp. 8vo.

8. *Bulletin of the Bussey Institution*, Vol. ii, Part 2. 80 pp. 8vo. Boston, 1877.—This number contains the following papers by F. H. Storer: (1) on the composition of certain Pumpkins and Squashes; (2) record of results obtained on analyzing seeds of Broom Corn; (3) record of analyses of several weeds that are occasionally used as food; Chemical composition of Blue Joint grass (*Calamagrostis Canadensis*) as contrasted with that of Reed Canary grass (*Phalaris arundinacea*); (4) remarks on American Fodder rations with hints for the improvement of some of them; (5) results obtained on growing Buck-wheat in equal weights of Pit-sand and of Coal-ashes; and the following by W. G. Farlow: Notes on some Common diseases caused by Fungi.

9. *Dynamics, or Theoretical Mechanics;* by J. T. Bottomley, M.A., F.R.S.E., F.C.S. 142 pp., small 8vo. New York, 1877. (G. P. Putnam & Sons.—This little work forms one of the volumes in "Putnam's Elementary Series." The fundamental principles of mechanics are presented with clearness and precision, without the introduction of difficult mathematics, and it is thus an excellent book for the class of students for whom it was prepared.

Eighth Annual Report of the State Board of Health of Massachusetts, January, 1877. 498 pp. 8vo, with several maps. A Report of high scientific value. The chief topics are: Pollution of streams; sewerage; sanitary condition of Lynn; registration of deaths and diseases; growth of children; disease of the mind (by Dr. C. F. Folsom); health of towns.

Science Lectures at South Kensington. The Steam Engine, by F. J. Bramwell. 62 pp. 12mo, with illustrations. London and New York. 1877. (Macmillan & Co.)

OBITUARY.

Sanborn Tenney.—Sanborn Tenney, Professor of Geology and Natural History in Williams College, Williamstown, Mass., died suddenly, on the eleventh of July, at Buchanan, Michigan, while on his way to Chicago, the point of rendezvous of a Williams College exploring party to the Rocky Mountains, of which he was the projector and leader. Professor Tenney was the author of valuable text-books on zoology and geology.

Dr. Stephen Reed.—Dr. Reed died on the twelfth of July at Pittsfield, Mass., aged seventy-six years. His name has been long connected with geological discovery in Western New England, and mainly through his account of a long train of bowlders across part of central Berkshire, which attracted Lyell and other geologists to the spot and has obtained a prominent announcement in all writings on North American drift.

C. F. Winslow, M.D.—Dr. Winslow, formerly of Boston, died recently in Utah, aged sixty-six years. He is the author of a work entitled "Cosmography, or Philosophical views of the Universe," published at Boston in 1853, also later of one on "Nature and Force;" and, besides, of short papers on some Earthquakes, in this Journal, and on the discoveries of human remains in California, and some other subjects.